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Rémi Catellier, Laurent Mazliak

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# The emergence of French probabilistic statistics

*Borel and the Institut Henri Poincaré around the 1920s*

Rémi CATELLIER<sup>1</sup> and Laurent MAZLIAK<sup>2</sup>

## Abstract

This paper concerns the emergence of modern mathematical statistics in France after the First World War. Emile Borel's achievements are presented, and especially his creation of two institutions where mathematical statistics was developed : the *Statistical Institute of Paris University*, (ISUP) in 1922 and above all the *Henri Poincaré Institute* (IHP) in 1928. At the IHP, a new journal *Annales de l'Institut Henri Poincaré* was created in 1931. We discuss the first papers in that journal dealing with mathematical statistics.

## INTRODUCTION

The important transformations in the field of mathematics of randomness between 1900 and 1930 are now rather well understood. Several large-scale studies have been published which present wide pictures of the actors and ideas involved in what may be considered as a major evolution in the scientific life of the 20th Century. See in particular several chapters in the two volumes of the monumental treatise [77] and fundamental book [121]. These books have been complemented by many recent papers concerning more detailed aspects of this story (see among numerous others [28], [66], [111] and other references in these papers). Yet it seems that these studies are often more specifically centered on the probabilistic aspects of the question than on the statistical side. When one consults the recent, very comprehensive collective work on statisticians edited by Heyde and Seneta [69], it is striking to see that many of those who are mostly known today as specialists in probability theory were also involved in the shaping of modern mathematical statistics. This includes for example the Austro-German Richard von Mises, the Soviets E.B. Slutsky, A.Y. Khinčin and A.N. Kolmogorov, the Italian Guido Cantelli and naturally, the French Emile Borel about whom we shall write at length below. Though basic tools of the modern theory of probability (especially the use of Borel's measure theory of sets and the Lebesgue integral) had been available since the 1910s in France, it took a long time for mathematics of randomness, above all the most modern ones, to penetrate the quite reluctant and suspicious world of field statisticians who preferred the use of descriptive methods without deep mathematical theory. The present paper approaches how eventually mathematical statistics slowly emerged in the 1920s. Let us mention that this question has already been studied, especially by Desrosières in his impressive book [45] where a wide panorama of the international situation is provided. More particularly, in France, we can also mention among others the paper [91]. Our paper seeks to provide further information on the French situation and its relations with what was happening abroad, and how the link was finally drawn between probability and statistics. As already mentioned, Emile Borel (1871-1956) was a central actor in this transformation, as were his successors Maurice Fréchet (1878-1973) and Georges Darmon (1888 -1961). By means

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1. ENS Cachan, France. remi.catellier@gmail.com

2. Laboratoire de Probabilités et Modèles aléatoires, Université Pierre et Marie Curie, Paris, France. laurent.mazliak@upmc.fr . The paper was completed when this author participated to a research program supported by the Institute of Communication Sciences of the CNRS.

of original pedagogical and scientific initiatives, the three men participated in the setting up of two institutes in Paris, the *Institut de Statistiques de l'Université de Paris* (ISUP) in 1922, and above all the *Institut Henri Poincaré* (IHP) in 1928 where mathematical statistics was presented and recent findings in the subject described for the first time in France. A good hint of the role played by the aforementioned mathematicians is that they were, between 1920 and 1950, the only mathematicians elected as President of Paris Statistical Society (SSP) (see [89]).

Our work stems from the conference organized in November 2008 by Michèle Audin and Catherine Goldstein for the 80th anniversary of the IHP. The second author was invited to present a talk on the same theme as the present article. Moreover, this talk was largely inspired by a short memoir [32] written by the first author after a short research stay at University Paris 6 during Spring 2008. On that occasion, important new sources on the subject (archival material and various articles) had been collected. This work was likewise strongly connected to a large international research program about the mathematics around the Great War, initiated 6 years ago. This program opened up numerous new directions for the historiography of this period, which were in particular discussed during an international conference in 2007 [81].

In 1996, Stigler ([114]) proposed the somewhat provocative hypothesis that Mathematical Statistics began in 1933. Some facts presented in our paper may therefore appear as a contradiction to Stigler's thesis. This is in fact only partly the case. Reading Stigler's paper shows that the author concentrates mainly on the case of the United States ; the obvious predominance of Anglo-Saxon statistics after the 1940s justifies particular attention on what happened in the USA during the interwar period. When we decided to scrutinize the French situation in the same period, we knew that we were looking at a country in which the role of mathematics in statistical studies was years behind in comparison with other countries. However, the actors of our story (Emile Borel and his successors) were mathematicians who used mathematics to teach and (timidly) invent statistical methods beginning already in the 1920s. As we shall see, it is clear that in their mind they were establishing a discipline called *mathematical statistics* in France - and *Mathematical Statistics* is precisely the title of Darmon's book in 1928 [37]. In fact, the point seems mainly to be a question of scale : Stigler's hypothesis pertains to a more macroscopic vision, so to speak. He himself mentions in his paper that it would certainly be more accurate to propose a confidence interval than to specify a precise year such as 1933. The fragments of the history of statistics we shall deal with in this paper may be considered as a part of the powerful stream that Stigler emphasizes, a stream which established mathematical statistics within the mathematical sciences.

The present paper is therefore an attempt to prove that Borel and his successors Darmon and Fréchet wanted to develop a type of statistics based on theoretical results of the calculus of probability. In their mind, the relationship between mathematical statistics and probability was therefore a *filiation* more than a complementarity. Moreover Fréchet, in his presidential address when he became President of the SSP in 1948, explicitly mentioned this idea and called calculus of probability the *father of mathematical statistics* ([52]). The three parts of our article discuss how the aforementioned program was implemented.

In the first part, we shall try to draw a picture of Emile Borel and his encounter with randomness. There are several texts presenting Borel and probability, and notably the important study by Callens [31]. However, it seems necessary to recall several facts about this major actor of our story in order to explain how his scientific and social personality contributed to the developments we discuss. Of particular importance for our subject were Borel's unceasing efforts to understand when and how probability could be applied legitimately to statistical situations. We believe that

the outlines of the mathematics taught in the new institutions during the 1920s and 1930s were strongly related to Borel's point of view. We insist on the role played by Borel's activities during the Great War. They provided him with a large-scale experience of applied statistics, especially through his various contacts with politicians.

The second part is devoted to the institutional realization of Borel's program. We deal with Borel's fundamental belief in the role of mathematical statistics in the education of decision-makers. We present how the setting up of the ISUP, and later of the IHP, was for him an opportunity to give shape to his ideas on education and scientific politics. We will dwell in particular on the roles played by Fréchet in Strasbourg and Darmon in Paris in the emergence of mathematical statistics during the 1920s in France. We will also provide detail on the functioning of the IHP and its scientific program.

The third part focusses on how the aforementioned institutions were used to implement what can be described as a *technology transfer* aimed at filling a gap in French statistical techniques by means of importation of foreign achievements. British, Scandinavian, German and Italian research was especially scrutinized. After having commented on the creation of the *Annales de l'Institut Henri Poincaré*, we concentrate on the first two papers on statistics published in the journal in 1932, the first one by Darmon, the second by Guldberg. The study of these papers, with a mathematical insight into their contents, is used as a demonstration of how the technology transfer worked. Moreover, the selection of subjects and speakers at the IHP shows how a kind of mathematical statistics *à la française* emerged based on the know-how of analysts and mathematical physicists converted to probability. It expanded during the 1930s as illustrated by Fréchet's polemics at the International Statistical Institute about the correlation (see [4]).

## 1. EMILE BOREL'S APPRENTICESHIP IN RANDOMNESS

Emile Borel, one of our main protagonists, was a major figure of the French mathematical scene in the first half of the 20th Century. His works were published under the care of his friend and former student Maurice Fréchet ([24]). On this occasion, Fréchet wrote a first biographical sketch, expressing his deep admiration for his former master. Since then, several biographies have been published about him. Let us note [98] and more recently [60].

Borel's mathematical achievements are so rich and offer so great a variety of subjects that it is obviously not the place here to propose a comprehensive picture of them. Besides, there is now a collection of works dealing with several aspects of Borel's mathematical production which will give the interested reader an idea not only of the results obtained by Borel but also of the spirit in which he produced them. We note for example the book [67] in which the genesis of the theory of measurable sets is presented in depth (a theory on which Lebesgue based the construction of his integral), or the paper [82] dealing with Borel's covering theorem (the compactness of the interval  $[a, b]$ ). The very recent paper [5] deals with philosophical aspects of Borel's consideration on approximation and with his criticisms of a notion of idealistic knowledge (a subject deeply related to ours as we shall see below).

The object of this first section is therefore to briefly recall how Borel became familiar with probability, and how randomness (in particular through a statistical orientation) acquired a considerable dimension in his vision of a scientifically enlightened citizenry and the rational direction of state business.

**1.1. Biographical sketch.** Emile Borel was born to a Protestant middle-class family in Saint-Affrique, in Aveyron, in the center of southwest France. He kept close ties with Saint-Affrique

throughout his life. After brilliant secondary studies, he went to Paris to sit for the competitive examinations leading to the Grandes Ecoles, the schools where the French scientific and administrative elites are trained. There he studied under the famous teacher Boleslas Niewenglowski, along with the son of mathematician Gaston Darboux, and he later recounted that it was at Darboux's home that he discovered his passion for scientific and especially mathematical research. The Ecole Normale Supérieure was the place where he pursued this passion.

However, purely mathematical achievements were only one side to the rich personality of Emile Borel. Very young, he engaged in an original mixture of academic, political and philosophical disputes, and had the strong desire not to be one of the type of mathematicians locked in university libraries, speaking only to his peers in his ivory tower. Borel was always convinced that the scientist had a role to play in the society of his time alongside politicians, writers, and artists, and he had the strong desire to participate in such social exchanges. Borel engaged himself on the social stage, especially through circles connected to the family of his talented wife Marguerite and his father-in-law, mathematician Paul Appell. Marguerite wrote fiction under the pen name Camille Marbo (for MARguerite BOREl) ; in 1913, she won the Femina prize for her novel *La statue voilée*. In 1905, Borel and Marbo founded a monthly journal, the *Revue du Mois*, which for 10 years was a leading general intellectual outlet for the moderate French left. When he became a politician in the 1920s, Borel, with his Protestant radical-socialist background, was passionately attached to secularism and rationalism, and was a typical representative of the *republic of professors* as journalist Albert Thibaudet later described the 3rd Republic [117]. However, in 1905, his ambition was at first only intellectual and the *Revue du Mois* became his favorite place for presenting his ideas (and his friends' views) on every kind of subject. Let us recall besides that this aspect of Borel's personality was not always accepted by his colleagues. Lebesgue, in particular, who had no great taste for the social commitments of scientists, criticized his behavior.

To speak the truth I reproach you for the *Revue du Mois*. I know quite well that you find there an opportunity to put into practice your qualities of action and your zeal as an administrator, but it is what I esteem least in you<sup>3</sup>. (*Lebesgue to Borel*, [20] February 1909 [49])

The interested reader can find details about the foundation of the journal in [84].

For several reasons, mixing an original turn in his mathematical reflection with his thoughts about how to enlighten his fellow man on a scientific basis, Borel soon had a kind of revelation that the mathematics of randomness could be an essential field for this struggle. Contrary to what is still sometimes asserted, Borel's reflections on probability do not belong to a second (and much less brilliant) scientific life beginning roughly in the 1920s. Borel met probability at the very beginning of the century and the subject always remained on his mind. He developed a complex relationship to it, one not always perfectly understood by his colleagues and in particular by younger French mathematicians, such as Paul Lévy, who became involved in the probabilistic field. Anyway, there is general agreement today that Borel should be seen as the harbinger of a renewal mathematical probability at the beginning of 20th century, opening the path to the axiomatic formalization based on measure theory achieved by Kolmogorov in his *Grundbegriffe der Wahrscheinlichkeitsrechnung* [107].

Several texts deal with Borel's original attitude towards randomness. Let us quote in particular [76] and obviously the seminal book [121] where Borel's probabilistic turn is studied in depth.

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3. Pour tout dire je vous reproche La Revue du Mois. Je sais bien que vous trouvez là l'occasion de dépenser vos qualités d'action et vos ardeurs d'administrateur, mais c'est ce que j'estime le moins chez vous.



The paper [29] provides another picture of this turn. Finally, in the second part of his PhD (published as [31]), Callens offers a profound examination of Borel's considerations on the role of quantified randomness in various aspect of social life. Much of what we write about Borel's life is inspired by these texts.

**1.2. Randomness.** When Borel entered the Ecole Normale, he immediately specialized in mathematics, beginning fundamental studies on divergent series, for which he introduced different modes of summability. This soon led him to fundamental work on the measure of sets, which cleared the way for Lebesgue to construct his integral and revolutionize analysis [67]. Measure theory also led Borel to focus on probability theory, starting in 1905 in [11]. Though we do not know exactly how Borel realized that measure theory and Lebesgue integral were perfectly adapted to probabilistic considerations, we can reasonably find two converging sources for his interest for this field. A first one is Wiman's paper [122] where, as Borel himself mentions in [11], measure theory was applied for the first time in a probabilistic context to obtain a limit distribution of the quotients of the continued fraction representation of a real number chosen at random between 0 and 1 (see details in [86]). It is worth recalling that Borel had introduced considerations on the measure of the sets in the first place (in his studies of analytic continuation [10] - see [67] pp. for details) as a powerful tool for proving the existence of a mathematical object. Borel's first reasoning in that direction was to assert that the complement of a negligible subset (i.e. one with null measure) of an interval must contain at least one point. As Boutroux wrote [27], Borel was a figure departing from a constructivist ideal, and he probably had the striking intuition that a non-constructivist attitude was extremely well adapted to describe mathematically the vague notion of randomness. In his thesis *Essay on Approximate Knowledge* (*Essai sur la connaissance approchée*), published in 1927 [6], Bachelard saw the use of probability (in particular by Borel) as an adequate tool for abstract speculations because probability is *a notion refractory to any ontology*. However, he was not completely right concerning Borel, for whom a reasoning involving probability provided a very concrete condition of existence, based on the precise meaning Borel had given to the notion of probability. As Darmois later wrote in [43], *until the beginning of XXth century, the very notion of probability remained rather vague. It is to Emile Borel that we owe its rigorous definition, based on the measure of sets*<sup>4</sup>.

But obviously the most convincing reason for Borel to be interested in probability should have been the increasing presence of probabilistic considerations in the new physics of his time, above all in statistical mechanics. Borel appears in this regard as a successor of Poincaré, who, after having tried to circumvent the cumbersome and shocking presence of randomness in physics, had finally made the best of the bad situation and decided, as he was no more in a position to avoid it, to give it a reasonable mathematical shape. After 1890, considerations on randomness and probability were always present in Poincaré's mind and he wrote the textbook [96] for his students at the Sorbonne, as well as several texts aimed at a more general audience published in his books (see in particular *Science and Hypothesis* [97]).

However, Borel (who belonged to the next generation) adopted a slightly different viewpoint from Poincaré's. For the latter what was at stake was to 'convince himself' that the probabilistic hypothesis was tolerable in physics (as Bru wrote in [28]).

As Barberousse comments in her paper [5], it seems that for Poincaré probabilism was reasonable only if it had a provisory character. For Borel, on the contrary, some situations (as the one he

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4. Jusqu'au début du XXème siècle, la notion même de probabilité restait assez vague. C'est à Emile Borel que l'on doit sa définition rigoureuse, basée sur la mesure d'ensemble

considers in his paper on the paradox of the wheat heap - see section 1.3 below) generate questions for which the adapted answer can only be given using a probabilistic modelization. It is therefore interesting to develop the mathematics of randomness in order to provide new tools for elaborating and studying these models.

Borel's first important probabilistic paper [12], devoted to a mathematical presentation for Maxwell-Boltzmann's kinetic theory of gases, was a magisterial illustration of such a program. He wrote

I would like to address all those who shared Bertrand's opinion about the kinetic theory of gases, that the problems of probability are similar to the problem of finding the captain's age when you know the height of the mainmast. If their scruples are partly justified because you cannot blame a mathematician with his love of rigor, it nevertheless does not seem to me impossible to make them happy. This is the aim of the following pages : they do not bring any real advance in the theory from the physical point of view ; but perhaps they will result in convincing several mathematicians of its interest, and, by increasing the number of researchers, will indirectly contribute to its development. If this is the case, they will not have been useless, independently of the esthetic interest connected with any logical construction<sup>5</sup>.

Borel's most important probabilistic achievement was the seminal paper [17] devoted to denumerable probabilities and how they could be used for arithmetical considerations. It remains probably one of the most important probabilistic papers ever done. The study of continued fractions, which had considerable consequences for modern probability theory - see [121], [86], appeared there once again. Borel stated there a first form of the strong law of large numbers, catching his contemporaries *by surprise* ([121], p.57) with this strange way of proving that almost every real number satisfied a property though it was hard to decide whether any particular number satisfied it. In fact, for Borel, such an assertion was a brilliant justification for his aforementioned intuition that a probabilistic answer was well adapted for providing a new modality of existence. The central point was to describe an impossibility by asserting that a set is negligible. This situation of almost impossibility or almost certitude was the only one in which one may attribute an objective value to probability. For Borel, the most important notion of probability theory (he would even call it later *La loi unique du hasard*, the unique law of randomness) was the interpretation that events with minute probability were impossible, as was illustrated by his famous popular image of the typing monkey reconstituting the books of the National Library. For Borel, the calculus of probability was seen as an application of mathematical analysis. One must therefore consider its results with the same caution as for any other application of mathematics. In particular, it is necessary to keep in mind that all the data we can collect contain imprecision. He wrote in [17]

It is obvious that such a theory cannot correspond to any real and concrete problem ; the conception of a denumerable sequence or unlimited sequence similar

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5. Je voudrais m'adresser à tous ceux qui, au sujet de la théorie cinétique des gaz, partagent l'opinion de Bertrand que les problèmes de probabilité sont semblables au problème de trouver l'âge du capitaine quand on connaît la hauteur du grand mât. Si leurs scrupules sont justifiés jusqu'à un certain point parce qu'on ne peut reprocher à un mathématicien son amour de la rigueur, il ne me semble cependant pas impossible de les contenter. C'est le but des pages qui suivent : elles ne font faire aucun progrès réel à la théorie du point de vue physique ; mais elles arriveront peut être à convaincre plusieurs mathématiciens de son intérêt, et, en augmentant le nombre de chercheurs, contribueront indirectement à son développement. Si c'est le cas, elles n'auront pas été inutiles, indépendamment de l'intérêt esthétique présent dans toute construction logique

to the indefinite sequence of integers is a purely mathematical and theoretical conception, and the related speculations are themselves part of mathematics. It is only in an indirect way that these speculations, which constantly involve the notion of infinity, may be able to get practical applications<sup>6</sup>.

Among numerous other examples, let us also quote the following sentence from his famous book 'Le Hasard' [18] in which he commented on the solution of a functional equation describing a probability

One should always accept with caution many results obtained in that way, even if the setting up of the functional equation cannot be criticized. One indeed is generally unaware of the extent to which a small error in a functional equation can imply considerable modifications in the results obtained with its solution<sup>7</sup>.

The strange kind of schizophrenia Borel developed towards probability had been the source of much comment. Borel was certainly the origin of a major renewal of the field of probability. He remained all his life a permanent supporter of the need for a probabilistic culture in society. But precisely for these reasons, the only possible justification for probability was for him its practical use. Therefore, the efforts needed to obtain a probability must be in direct connection with their practical importance. Because this importance must always be wisely considered, Borel became reluctant to use high mathematics in any application, and first of all probability. This fact is maybe best illustrated by his own lack of consideration for the discipline - at least after WW1 when he told his wife that because high mathematics cracked his skull, he lazed about (in French *pan-touflait*) in probability. Whether Camille Marbo's account was true or not, it is clear that Borel's viewpoint placed him in an awkward position with the new probabilistic generation. In particular, Borel's attitude profoundly shocked Paul Lévy who became, as is well known, the major French specialist in probability of the inter-war period. For Lévy, mathematical constructions, based on sophisticated techniques if required, were necessary to guarantee the validity of the application of probability theory. On this subject, consult [8].

Considerations on randomness, probability and statistics were an important topic for papers from the beginning, especially Borel's in the *Revue du Mois*, the journal he founded with his wife in 1905. Borel found in it the exact tool he needed to improve his conceptions on these subjects. The first paper of the journal, written by Volterra<sup>8</sup> [119], gave a hint of what Borel hoped to do : *For most mathematicians wrote Volterra, the desire emerges to lead their minds beyond the limits of pure mathematical analysis*<sup>9</sup>. Let us observe that Volterra quotes Pearson and the achievements

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6. Il n'est pas besoin d'observer qu'une telle théorie ne peut correspondre à aucun problème réel et concret ; la conception d'une suite dénombrable ou suite illimitée analogue à une suite indéfinie des nombres entiers est une conception purement mathématique et théorique, et les spéculations qui s'y rattachent sont elles-mêmes du domaine propre des mathématiques. C'est seulement d'une manière indirecte que ces spéculations, où intervient à chaque instant la notion de l'infini, pourront se révéler susceptibles d'applications pratiques.

7. On doit toujours accepter avec circonspection beaucoup de résultats obtenus de cette manière même si l'établissement de l'équation fonctionnelle ne peut pas être critiquée. On ignore en effet en général à quel point une petite erreur peut entraîner des modifications considérables dans les résultats obtenus avec sa solution.

8. About this article, we can consult the detailed study by Durand and Mazliak [50], which argues that the Prolusione was the Borel's first encounter with the kind of applied mathematics represented by biometrics. Volterra's quick affirmations about the role of probability and statistics in a near future had an influence on Borel's program. The fact that this article was the first published in the *Revue du Mois* gives it a strong programmatic nature.

9. Chez la plupart des mathématiciens s'éveille le désir naturel de diriger leur esprit hors du cercle de la pure analyse mathématique.



of the British statistical school as an answer to a situation where the increasing complexity of biological knowledge makes Claude Bernard's experimental method on particular cases impractical. The biometric school provided statistical methods to overcome the necessary renunciation of *the hope of obtaining a mathematical relation between each ancestor and each descendent*<sup>10</sup>. A hint of Borel's interest in Pearson's work is his publication in 1908 of a rather unnoticed note to the Comptes-Rendus in the section *Mathematical Statistics*, devoted to considerations about Pearson's polymorphic curves ([16]).

**1.3. A social science : Discussions and Polemics.** Borel's first intervention on probability in the *Revue du Mois* is [13]. The question of practical value was to remain at the core of Borel's philosophy of mathematical randomness until the end of his life. In practical life, Borel writes, probabilistic computations *put some elements of our decisions under a form easier to grasp. They replace some more or less complex data by a small number of simple figures*<sup>11</sup>. Borel's main problem is therefore the scientific interpretation of statistical facts which are no longer considered only as a collection of descriptive data.

In subsequent papers in the *Revue du Mois* (which became a major basis for the book *Le Hasard* [18]), Borel was to develop these considerations. Let us in particular comment on two papers.

Paradoxes often appear in economic life. For instance, let us suppose that a milkman must decide on the retail price for one liter of milk if the wholesale price has been increased by 0.5 cent. If he decides to pass the cost along to his customers, he must necessarily amplify the increase of the retail price to at least one cent and may face a loss of customers. On the contrary he may accept a slight reduction of his profit until a subsequent increase of the wholesale price which would allow him to pass both increases to the retail price. There is no logical way to decide which choice is the best one. In [14], Borel explained how a probability distribution obtained from statistical observation is the only satisfying answer. Borel connects the question with Zeno's classical paradox of the heap of wheat : when do we decide that a certain number of grains of wheat constitute a heap ? For Borel, again only a statistical answer is reasonable, through the interrogation of a sample of people. He wrote

The idea I would like to extract from what precedes, is that the adequate mathematical answer to numerous practical questions is a probability coefficient. Such an answer may not seem satisfactory to many minds who expect certainty from mathematics. It is a very unfortunate tendency : it is mostly regrettable that public education be, in that respect, so little developed ; it is probably due to the fact that probability calculus is almost universally unknown, though it gradually penetrates everybody's life (though various forms of insurance, mutual funds, pensions and so on). A probability coefficient constitutes a definitely clear answer, corresponding to an absolutely tangible reality. Some minds will maintain that they 'prefer' certainty ; perhaps they also would prefer that 2 plus 2 were 5<sup>12</sup>.

10. L'espérance d'obtenir une relation mathématique entre chaque ancêtre et chaque descendant.

11. mettent sous une forme plus facile à saisir certains éléments de nos décisions. Ils remplacent certaines données plus ou moins complexes par un petit nombre de chiffres simples.

12. L'idée que je voudrais dégager de ce qui précède, c'est que la réponse mathématique à donner à bien des questions pratiques est un coefficient de probabilité. Une telle réponse ne paraîtra pas satisfaisante à bien des esprits, qui attendent des mathématiques la certitude. C'est là une tendance très fâcheuse ; il est extrêmement regrettable que l'éducation du public soit, à ce point de vue, si peu avancée ; cela tient sans doute à ce que le calcul des probabilités est à peu près universellement ignoré, bien qu'il pénètre chaque jour davantage dans la vie de chacun

In a subsequent paper ([15]) Borel tried to demonstrate that better education on probability would be a good way to improve social solidarity and to avoid a *kind of individualism which is nothing but stupid selfishness*. For Borel, probability is the basis of what may be called *social mathematics*. One may state statistical laws satisfied by the facts, and such laws have the power to limit the excesses of individualism and therefore to contribute to the development of solidarity. For Borel quantification of randomness was a perfectly scientific attitude and he was probably surprised at stubborn reactions to this conception. He wrote :

Trying to go in depth into the reasons why the calculus of probability is unpleasant to many minds, I hope to succeed in showing that this unpleasantness is largely based on misunderstanding ; I wish this misunderstanding were removed, for the popularization of the conclusions, if not of the methods of this branch of science, could have much social utility. (...) In general, men do not like to lose their names and to be represented by a number ; neither do they like to be considered only a unit in a group without being individually identified. Here is already the reason why statistics are not popular and why easy jokes about them are generally welcomed. (...) [The calculus of probability], not only lists past events, but also claims to predict future events to some extent ; here lies its scientific aspect. This claim shocks the psychological sense of human freedom in the first place. (...) One must not fear computations, if one is ready not to behave according to its results without having firstly weighted them for what they are : it is a strange illusion to think that individual independence is increased by ignorance. (...) Someone who is starving is not much interested in the increase of average fortune : one must not seek in statistics or in computations arguments for the consolation of those who suffer from social inequalities ; but this observation does not diminish anyhow the proper value of statistics or of the computations one uses to interpret them<sup>13</sup>.

We see how Borel followed Quetelet, Cournot and Markov's path in their quarrels with the opponents to the conception of an 'average man', allegedly in contradiction to man's free will, whether a religious tenet or not : see [31], [90] and [106] on this subject.

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(assurances diverses, mutualités, retraites, etc.). Un coefficient de probabilité constitue une réponse tout à fait claire, correspondant à une réalité absolument tangible. Certains esprits maintiendront qu'ils "préfèrent" la certitude ; ils préféreraient peut-être aussi que 2 et 2 fissent 5.

13. En essayant d'approfondir les raisons pour lesquelles le calcul des probabilités est antipathique à beaucoup d'esprits, j'espère arriver à faire voir que cette antipathie repose en grande partie sur un malentendu ; il serait désirable que ce malentendu soit dissipé, car la vulgarisation des conclusions, sinon des méthodes de cette branche de la science, serait d'une grande utilité sociale. (...) [L]'homme n'aime pas, en général, perdre son nom et être désigné par un numéro ; ni même être compté seulement comme une unité dans un groupe sans être individuellement désigné. C'est déjà là une raison pour que la statistique ne soit pas populaire et pour que les plaisanteries faciles que l'on peut faire à son sujet soient généralement bien accueillies. (...) Le calcul des probabilités (...) non content de recenser les événements passés (...) prétend prévoir dans une certaine mesure les événements futurs : c'est en cela qu'il est une science. Cette prétention heurte tout d'abord le sentiment psychologique de la liberté humaine. (...) On n'a rien à redouter du calcul, lorsqu'on est décidé à ne pas régler sa conduite sur ses indications sans les avoir au préalable pesées à leur juste valeur : c'est une illusion singulière de penser que l'indépendance individuelle est accrue par l'ignorance. (...) Celui qui meurt de faim s'intéresse peu à l'augmentation de la fortune moyenne : on ne doit pas chercher dans la statistique ni dans le calcul des arguments pour consoler ceux qui souffrent des inégalités sociales ; mais cette constatation ne diminue en rien la valeur propre des statistiques ni des calculs par lesquels on les interprète.

Borel was never really fond of abstract speculations about the meaning of probabilistic statements. In 1909 he published his *Eléments de la thorie des probabilités*, where he gave an exposition of the mathematical results forming the basis of his theory of probability. The book attracted Keynes's misunderstanding. In two violent reviews ([74] and [75]), Keynes accused Borel of neglecting the philosophical interpretation of his results and of being concerned only by the mathematical side. Reading what Keynes wrote, it seems that Borel was for him typical of the French way of considering these problems. Borel's answer came only fourteen years later (revenge is a cold dish !) when he strongly criticized Keynes in the review he wrote about the latter's 1921 treatise on Probability for the *Revue Philosophique* in 1924 [22]. The amazingly harsh words used by Borel in what is usually a rather conventional exercise are certainly a hint that he was irritated by Keynes' obsession with the interpretation of results in probability at the expense of their use as scientific results (Keynes' aim in his treatise was to obtain a satisfactory basis for a logical interpretation of probability). Borel was in particular upset that Keynes did not even mention statistical mechanics and the kinetic theory of gases which were for him, as we have seen, the paradigmatic examples which made randomness unavoidable in modern scientific conceptions. Borel wrote : *Maxwell is one of the most celebrated names the famous Cambridge university can be proud of, a university to which M.Keynes insists on connecting his intellectual education. If he is left aside, it is certainly not out of ignorance, but because systematically*<sup>14</sup> and later : *Is the highly successful application of probability to [physical] questions devoid of reality in M.Keynes' eyes ?*<sup>15</sup> Borel even went as far as considering Keynes' ideas as typical of the British way of thinking. As a conclusion to his introduction, Borel wrote the following lines :

This proves once again how different British minds are from continental ones ; we must neither be hypnotized by these differences nor look with obstinacy for what is to us incomprehensible ; it is better to admit these differences as a *matter of fact*<sup>16</sup> and nevertheless try to adapt the original ideas of the British to our particular mentality. By doing so, we are almost certain to betray them, but at the same time we give to them the only chance to exert an influence on minds built differently from theirs. The history of science shows that this collaboration between minds which do not understand each other perfectly is not only possible, but often fruitful. I strove to understand M.Keynes and, whenever I felt myself too far from him, I strove to faithfully look for the ideas, however different from his they may be, which had inspired me on reading his book<sup>17</sup>.

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14. Maxwell est cependant un des noms les plus illustres dont puisse s'enorgueillir la célèbre Université de Cambridge, à laquelle M.Keynes tient à rattacher sa formation intellectuelle. S'il est laissé de côté, ce n'est certainement pas par ignorance ni par oubli mais en vertu d'un système.

15. Est-ce l'application pleine de succès du calcul des probabilités à ces questions qui est dépourvue de réalité aux yeux de M.Keynes ?

16. In English, in the original !

17. Ceci prouve une fois de plus combien sont différents les esprits des Anglais et les esprits des continentaux ; nous ne devons pas nous hypnotiser sur ces différences et chercher avec obstination à comprendre ce qui est pour nous incompréhensible ; il vaut mieux admettre ces différences comme une *matter of fact* et essayer néanmoins d'adapter à notre mentalité particulière les idées originales des Anglais. Ce faisant, nous sommes à peu près sûrs de les trahir, mais en même temps de leur donner la seule chance qu'ils peuvent avoir d'exercer une influence sur des esprits faits autrement que les leurs. L'histoire des sciences montre que cette collaboration entre esprits qui ne se comprennent pas complètement est non seulement possible, mais souvent féconde. Je me suis efforcé de comprendre M.Keynes et, lorsque je me sentais trop éloigné de lui, de rechercher loyalement quelles idées, peut-être fort différentes des siennes, m'étaient suggérées par la lecture de son livre.

As can be seen, Borel was not a man afraid of controversies. On the contrary, the *Revue du Mois* had been the battlefield between Borel and several scholars of the time. One of the most important controversies was the recurrent discussion with biologist Le Dantec. As a brilliant mind, Le Dantec was a firm opponent of a scientific conception of probability. Above all he objected to letting such a conception invade the domain of the life sciences. In 1910, Le Dantec wrote a paper called *Mathematicians and probability* [79]

I will teach everything necessary for actuaries, for the kinetic theory of gases, and so on, without having ever pronounced the dangerous word of probability or chance ; I would rather call this part of mathematics : the computation of means, in the case of phenomena which are never submitted to any law<sup>18</sup>

We shall not comment further on Le Dantec's discussion with Borel which is thoroughly analyzed in [29].

Another discussion was Borel's direct opposition to Bergson's conception of education. This theme was examined by Callens in [31]. We will just recall some of its basic features. The French philosopher had adopted a characteristic anti-intellectual attitude and defended the idea that an *intellectual man* deprives himself of the natural intelligence which alone can provide tools for an adaptation to the environment. An improvement of this natural intelligence can only be achieved through classical studies (Greek, Latin, geometry. . . ) which allow the selection of an elite of men of action. As Grivet wrote when he commented on Bergson's philosophy of action : *Forget about adjusting your action to knowledge, or you will condemn yourself to never do anything. The man of action is the one who, at a given time, knows how to silence the faculty of reasoning.*<sup>19</sup> Obviously, quantifying randomness is quite unnatural to the human mind and therefore does not fit Bergson's conceptions. For Borel, on the contrary, it is precisely from data mining and statistical treatment that the good sailor can prepare for the storm. The practical value of probability for Borel is that computation allows one not to bury one's head in the sand. It is therefore the best reliable support for a clever and brave attitude in face of danger. Bergson advocates that one should follow one's instinct. But, Borel asserts, instinct deceives - and such a behavior would lead to an attitude much less adapted to life than being trained to risk. Borel finds a proof of this deceiving aspect of instinct in the way men eventually had to renounce a completely mechanical explanation of the universe. For example, *[t]he discovery and study of radioactivity showed that mechanical explanations are sometimes certainly insufficient and must give way to statistical explanations*<sup>20</sup>. Much later, when Borel wrote the final part of the long series of volumes on probability theory he edited, which he devoted to the *Practical and philosophical value of probability*, he summed up the views he had developed in his previous publications. *Probability calculus is essentially social science*<sup>21</sup> which allows the simulation of the social effects of a decision. This is profoundly related to the fact that probability uses sets and populations and not individuals as basic components. Thus *the theory of probability dominates all experimental science as deductive logic dominates*

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18. J'enseignerai ainsi tout ce qui est nécessaire pour les actuaires, pour la théorie cinétique des gaz, etc., sans avoir jamais prononcé le mot dangereux de probabilité ou de chance ; j'appellerais volontiers cette partie des mathématiques : le calcul des moyennes, dans le cas des phénomènes qui ne sont jamais soumis à aucune loi

19. Renoncez à régler votre action sur la connaissance, ou vous vous exposerez à ne jamais rien faire. L'homme d'action est celui qui, à un moment donné, sait faire taire au plus vite la faculté de raisonner ([58], pp.471-472).

20. L'explication mécanique de l'univers s'est toujours dérobée jusqu'au jour où la découverte et l'étude de la radioactivité ont montré que les explications mécaniques sont parfois certainement insuffisantes et doivent alors céder le pas aux explications statistiques ([18], p.iii)

21. Le calcul des probabilités est essentiellement une science sociale ([23], p.129)

*mathematical science*<sup>22</sup>. Probability is in fact the only way to give a meaning to a collection of data. Borel is extremely assertive in his conclusion : *The value of any science is founded on reasoning with probabilities.*<sup>23</sup>.

As we have seen, in 1905 Borel began his interest in probability in a purely mathematical way - the application of measure theory. But, as the practical and social value of mathematics had always been a concern for him, questions about justifying the application of probability theory to statistics - which deals with the concrete aspect of random phenomena - naturally came soon. An exchange with Lucien March (the head of the Statistique Générale de France) in 1907<sup>24</sup> testified to this fact. March explained that the core of the application is the assimilation between observed frequencies and probabilities. This could certainly be sometimes true but the problem had not been studied enough. In the next years, Borel was led to see some probabilistic results as the theoretical basis for the study of statistical situations. Probability became therefore in Borel's mind this "father of mathematical statistics" (père de la statistique mathématique), described later by Fréchet in 1948 ([52]). Borel was to advocate this idea in his own 1923 presidential speech. He provided the law of errors as main example, the use of which he considered universal in statistics ([21]). Not coincidentally, in the same year, Borel and Deltheil wrote the little book *Probabilités, Erreurs* ([25]) where they aimed at presenting the Central Limit Theorem and some of its applications with the least mathematical technique.

**1.4. The war experience.** The sudden outbreak of the Great War was the major event which allowed Borel to make a kind of full-sized test of his former considerations on randomness and statistics. In a paper [19] written in 1919 for the revival of the *Revue du Mois*<sup>25</sup>, Borel wrote that *the Great War has been a decisive experiment as well as an education*<sup>26</sup> to help us realize that science had a major role in the material development of mankind, an idea commonly accepted during the 19th Century but mostly limited to intellectual considerations that *had not deeply penetrated the intimate depths of consciousness*<sup>27</sup>.

The recently edited war-correspondence between Volterra and Borel ([88]) provides an impressive picture of how, since the very beginning, Borel had decided to involve himself in the war effort, though at the age of 43 he could have made a more comfortable choice. But in fact, as some comments in [88] try to illustrate, Borel's conception of social life prevented him from even thinking of the possibility of staying on the sidelines. Just as the mathematician needed to be implied in the life of the city (and Borel's leitmotiv was that probability theory gave a tool for that to mathematicians), the scientist could not help playing his part in the war. Borel enlisted in the army in 1915 and participated in the testing of sound ranging apparatus, in particular the so-called Cotton-Weiss apparatus devised in 1914 by physicists Aimé Cotton and Pierre Weiss for the localization of artillery batteries, on the front.

But probably more significant to the story we present here, Borel was soon proposed to take up important responsibilities at a high governmental level, close to the center of power in the very

22. la théorie des probabilités domine toute la science expérimentale, autant que la logique déductive domine la science mathématique ([23], p.126)

23. La valeur de toute science a comme fondement des raisonnements de probabilités [23], p.126

24. Letter from March to Borel, 16 July 1907. Archive of Paris Academy of Sciences.

25. The publication of the journal in fact lasted only one year, mostly because the energy and the faith of the beginning had vanished.

26. la grande guerre a été une épreuve décisive et en même temps un enseignement

27. n'avait pas pénétré dans les profondeurs intimes de la conscience



centralized France of that time. In November 1915, his friend and colleague mathematician, Paul Painlevé, minister for public instruction, decided to set up a special service connected to his ministry in order to gather the various enterprises dealing with technical research into the war effort. Painlevé and Borel were very close friends, belonging to the same political family and sharing common views on the role of the engaged scientist in the city. Painlevé contributed several times to the *Revue du Mois* ; in 1913, together with Borel and Charles Maurain, he wrote a visionary book on the development of aircraft [26]. Painlevé asked Borel to head the special service called the *Direction des inventions intéressant la défense nationale* (Direction of Inventions related to national defense). A clipping from the *Journal de Paris* on 15 November 1915 asserts that Borel's nomination

as head of the service of inventions will be welcomed with general satisfaction. He is known in the scientific world for his works of pure mathematics and mechanics. Also, he has already collaborated with the present minister for public education in studies about aviation<sup>28</sup>.

The journalist obviously could not have been aware of the importance of Borel's reflections on statistics and randomness though they were probably constantly on Borel's mind. The same year Borel was chosen to be a member of the SSP board. His new function as head of the *Direction des Inventions* was probably the main reason for this choice, but Borel's interest in statistics was certainly taken into consideration. When, two years later, Painlevé became Prime Minister (*Président du Conseil* - President of the council, during the 3rd Republic) and Borel was promoted as head of Cabinet, he assigned himself a major task : to organize rationally the statistical data mining devoted to helping the Prime Minister in his decisions. In fact, Borel did not have enough time for that. Three months after his arrival as Prime Minister, in November 1917, Painlevé's government was toppled as a consequence of the Italian disaster of Caporetto (see [88] for details), and Borel returned to his experiments on sound ranging on the front. However, this experience made a profound impression on him and just after the war, he wrote a text, published in the Journal of Paris Statistical Society in January 1920, entitled *Statistics and the Organization of the Presidency of the Council* ([20]). In this text, Borel wrote the following lines directly inspired from his observations when working with Painlevé.

I would like to insist on the role of the service depending on the Presidency of Council which we can call, to specify its nature, a statistical cabinet, as it is for me this statistical cabinet that befalls one of the most important and at the same time one of the most delicate tasks of the government of the country. The number and the material importance of statistical documents increase each day in every country ; one realizes better, indeed, the importance of obtaining sufficiently detailed statistics in order to use them in different situations. Social phenomena are too complex to be easily included in oversimplified formulae. But, on the other hand, reading and interpreting considerable statistical documents require not only some specific form of education but also a large amount of time. We must admit that the heads of Government are not short of specialized education but they are mainly short of time. It is therefore necessary that men in which they have

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28. Sa nomination à la tête du service des inventions sera accueillie avec une satisfaction unanime. Il est connu dans le monde scientifique pour ses travaux de mathématique pure et de mécanique. Enfin, il a collaboré déjà avec le ministre actuel de l'instruction publique, dans les études que celui-ci a faites sur l'aviation

full trust sum up and interpret statistical documents for them. But, once data are summed up and interpreted, any rigorously scientific and objective work is impossible ; it is therefore not possible to entrust civil servants with this work whatever their professional value can be, as their opinions may be on such or such question of economic, customs or fiscal politics, opposed to the Government's opinion. (...) Here is not the place to insist on the fact that statistics is an indispensable aid to all those who are in charge of the heavy task of ruling a country. If nevertheless we all agree on the principle, there may be divergences of opinion on the most favorable modes of carrying things out, that may lead to a very profitable discussion. The times seem to me particularly well suited for this discussion as within some weeks, France will choose the Government which will organize peace after choosing the Governments which have won the war. Without involving ourselves here in political matters, we can however assert that whatever the Government will be, a clever use of statistics will be useful to them. (...) Let me remind you, as an example of what can be done in that direction, of the organization of the technical services of the War cabinet such as Painlevé conceived it. The numerous statistical documents related to the war policies (French, allies or enemy troop sizes, losses, ammunition, necessary and available tonnages, submarine warfare, exchange rates and so on) were collected and summed up. A notebook, called the black notebook, was constituted ; it was formed of around ten cardboard sheets, equipped with tabs and also a folder. By opening the notebook on the page of troop sizes for instance, one immediately found the current, summarized information in the specific form required by the minister on a sheet of paper updated each week ; in the corresponding folder, were collected retrospective information, further details, graphs. (...) When M. Painlevé became also Head of the Government, a grey notebook, of the same kind and containing statistical documents of interest for the economic committee, was joined to the black one which contained the statistics of interest for the war committee<sup>29</sup>.

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29. Je voudrais (...) insister un peu sur le rôle de l'organe de la présidence du Conseil que nous pouvons appeler, pour préciser sa nature, cabinet statistique, car c'est, à mon avis, à ce cabinet statistique que doit incomber une des tâches les plus importantes et en même temps les plus délicates dans le gouvernement du pays. Le nombre et l'importance matérielle des documents statistiques augmente chaque jour dans tous les pays ; on se rend mieux compte, en effet, de l'importance qu'il y a à posséder des statistiques suffisamment détaillées pour qu'elles soient utilisables à des fins diverses. Les phénomènes sociaux sont trop complexes pour qu'il soit possible de les enfermer dans des formules trop simplifiées. Mais d'autre part, pour lire et interpréter des documents statistiques considérables, il faut, non seulement une éducation spéciale, mais beaucoup de temps. Nous devons admettre que l'éducation spéciale ne fait pas défaut aux chefs du Gouvernement, mais c'est le temps qui leur manque le plus. Il est donc nécessaire que les hommes en qui ils aient pleine confiance résument et interprètent pour eux les documents statistiques. Or, dès qu'il y a résumé et interprétation, il ne peut plus être question d'un travail rigoureusement scientifique et objectif ; il n'est donc pas possible de confier ce travail à des fonctionnaires quelle que soit leur valeur professionnelle, dont les vues personnelles peuvent être, sur telle question de politique économique, douanière ou fiscale, en opposition avec celles de Gouvernement. (...) [C]e n'est pas ici qu'il est nécessaire d'insister sur le fait que la statistique est un auxiliaire indispensable pour ceux qui assument la lourde tâche de gouverner un pays. Si cependant nous sommes tous d'accord sur le principe, il peut y avoir sur les modes d'exécution les plus favorables, des divergences d'appréciation qui pourraient conduire à une discussion très profitable. Le moment me paraît particulièrement bien choisi pour cette discussion car c'est dans quelques semaines que la France va, après les Gouvernements qui ont gagné la guerre, connaître les Gouvernements qui organiseront la paix. Nous n'avons pas à intervenir ici dans les questions politiques, mais nous pouvons affirmer que, quels que soient ces Gouvernements, l'emploi judicieux des statistiques

After he became a politician in the classical meaning of the word (first as Mayor of St Affrique in 1923, then as Deputy in 1924 - he was even a minister for the Navy in Painlevé's (once again short) presidency of the Council in 1925), Borel tried to obtain the creation of a service of statistical documentation and economic studies. He pleaded therefore for the *institution of data, given that the 19th Century, in its romanticism, had only established an institution of facts*<sup>30</sup>. In his posthumous comments on Borel's works, Denjoy wrote that he doubted *that a single analyst ever had a keener sense of numerical reality than Borel*<sup>31</sup> ([44]).

The war experience had been for Borel a large-scale numerical experience. This was not only because total warfare created a situation where enormous quantities of materiel, food, and weapons were used, replaced, and exchanged, and in which the unprecedented size of the armies made it necessary to develop tools for the direction of millions of soldiers; but also because the trauma caused by the huge losses forced the forging of a new conception of social life. We refer the reader to [88] and [87] for a presentation (and some affecting documents) on how Borel faced the tragedy - a tragedy which besides affected him personally through the loss of his foster son Fernand as well as it affected his fellow mathematicians Hadamard, Picard and so many others. For Borel, social mathematics belonged to the tools provided to society to avoid the woes created by man's natural selfishness. It remains profoundly linked to a constitutional vision of politics, close to the ideals promoted by the social-radical politicians of his generation, providing a tool whose purpose was among other things to keep any authoritarian tendency at bay. In a book written in 1925, whose title *Organize* is in itself a program, Borel advocated an international law to which each State must be submitted. Each State must be accountable to public opinion and to an assembly of nations. A combination of exchanges between *assemblies of free men*, seen as a combination of economic, cultural, and political exchanges is seen as the best guarantee against the *most absolute powers*. Some letters exchanged with Volterra (who entered very soon into direct opposition to Mussolini's regime - see [57] and [59]) bear testimony of how the Italian events at the beginning of the 1920s were for Borel an illustration of the aforementioned necessity. We shall see in the next section how the foundation of the Henri Poincaré Institute was used by Borel as an opportunity to invite fellow scientists trapped in the convulsions of the interwar period in Europe.

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leur sera nécessaire. (...) Qu'il me soit permis de rappeler, comme exemple de ce qui peut être fait dans ce sens, l'organisation des services techniques du Cabinet au ministère de la Guerre, telle que l'avait conçue M. Painlevé. Les très nombreux documents statistiques intéressant la politique de guerre (effectifs français, alliés ou ennemis, pertes, munitions, tonnages nécessaires et disponibles, guerre sous-marine, changes, etc.) étaient rassemblés et résumés. Un cahier, que l'on appelait le cahier noir, avait été constitué; il était formé d'une dizaine de feuilles de carton, munies d'onglets et dont chacune comportait en outre une pochette. En ouvrant le cahier à la page des effectifs, par exemple, on trouvait immédiatement sur une feuille renouvelée chaque semaine, les renseignements actuels résumés sous la forme désirée par le ministre; dans la pochette correspondante se trouvaient des renseignements rétrospectifs, des détails complémentaires, des graphiques. (...) Lorsque M. Painlevé joignit la présidence du Conseil au ministère de la Guerre, au cahier noir qui contenait les statistiques intéressant le Comité de Guerre fut adjoint un "cahier gris" établi sur le même modèle et renfermant les documents statistiques intéressant le Comité économique.

30. pour l'institution des données, alors que le XIX<sup>ème</sup> siècle, par romantisme, n'avait réalisé qu'une institution des faits.

31. Je doute qu'un seul analyste ait eu autant que Borel, ni même à un degré comparable au sien le sens de la réalité numérique

## 2. THE EMERGENCE OF MATHEMATICAL STATISTICS IN FRANCE

In 1920, Borel, who occupied the chair of function theory asked the University of Paris to be transferred to the chair of Probability Calculus and Mathematical Physics. This happened at the precise moment when he decided to resign from the position of vice-director of the Ecole Normale Supérieure, because, wrote Marbo in her memoirs ([83]), he could not any longer face the ghosts of all the young students fallen during the war (see [86]). We have already mentioned how Marbo commented on Borel's weariness about higher mathematics after the war. Several commentators take this assertion for granted and want to see Borel's desire for transfer as a proof of this lack of interest, due to his ambivalent relationship towards probability as a mathematical theory. Nevertheless, in a letter to Volterra ([88], p.138), Borel told his Italian colleague that he had tried to convince Langevin to accept the position. Had the latter accepted, it would be interesting to understand what Borel would have done concerning his own situation. But Langevin had refused and Borel asked for the position which he also probably felt as a convenient place to develop his ideas on randomness. Gispert ([55]) and Gispert and Leloup [56] have observed that the discussion about Borel's transfer was the occasion of a new crisis between Borel and Lebesgue after their break in 1917, Borel having proposed the transformation of his chair of theory of functions into a chair of theoretical and celestial physics. Lebesgue was upset because he saw this as a betrayal of mathematics. He wrote : *Should M.Borel succeed - and he will - his lectures will attract physicists and if he attracts also mathematicians, they will be lost for mathematics*, illustrating the poor opinion he himself had of probability.

Borel was intent in doing all that was possible to improve the diffusion of the mathematics of randomness in France. A trace of these efforts can be found in the introduction to the small textbook he wrote with Deltheil ([25]).

It is only for reasons of tradition, shall we say of routine, that the elements of probability calculus are not present in the syllabus of secondary schools, where they could with benefit replace many of the remaining subjects, which are still there only because no one thought of getting rid of them<sup>32</sup>.

**2.1. Strasbourg.** An important lesson of the war had been the observation of the efficiency of German organization. During the entire 19th Century, and especially after the birth of the German Empire, the development of Germany had been accompanied by a development of powerful statistical institutions (see [45], p.218 to 231). As we have seen, Borel also knew about the active school of biometrics in Great Britain around Karl Pearson. By contrast, in France, the scene of academic mathematical statistics was desperately empty.

Just after the war, the discipline experienced a promising development in Strasbourg, which had just been returned to France.

In 1919, Fréchet was sent to the University of Strasbourg, and came there as a missionary of science. The Government wanted the institution to be a showcase of French research success. Deputy Manoury wrote the following letter on 5 April 1919 to the French government representative Alexandre Millerand :

You know better than anyone the considerable importance given by the Germans to this university and the special attention they paid to making it one of the most

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32. C'est uniquement pour des raisons de tradition - l'on n'ose écrire de routine - que les éléments du calcul des probabilités ne figurent pas au programme de l'enseignement secondaire, où ils remplaceraient avantageusement bien des matières qui y subsistent pour le seul motif que personne ne se donne la peine de les supprimer.

brilliant, if not the brightest, of all the universities in the Empire. You have certainly also seen that they predicted that in less than 3 years France would have sabotaged their work. How could we face this challenge ?<sup>33</sup>

On the university of Strasbourg at that time, we confer the reader to the book [35], and more precisely to Siegmund-Schultze's text about Fréchet [110].

Thus, Strasbourg University had become within 10 years a first-rank institution in France, and a place of original intellectual experiments. During the Imperial period, Straßburg had indeed been an important place for statistical research, with Lexis and Knapp. The young statistician Henri Bunle was sent to Strasbourg to retrieve German technical knowledge, as he explained in an exciting interview in 1982 ([46]). Desrosières asked Bunle what his task in Strasbourg was in 1919, and Bunle explained that his mission was to control the German statistical bureau which published a statistical directory for Alsace-Lorraine where 7 or 8 people worked. Bunle said :

I recruited Alsatians and Lothringians with deep [local] roots. I put these guys next to [the Germans]. I went to see the Germans and said : I have placed people from Alsace and Lorraine beside you so that you can explain to them thoroughly what you are doing. You will leave only when these people tell me that they know the trade. Thus, as they wanted to go away, everything turned out OK.<sup>34</sup>

Among the new pedagogical initiatives in Strasbourg, there was the creation of the *Institut d'études commerciales* (Institute for Commercial Studies) where Fréchet and the sociologist Maurice Halbwachs taught, as soon as 1920. Later, they published a book on their common experience ([53]). In the preface they explain the purpose of the book. For Fréchet, who was in charge of the lectures on insurance, the scientist who is deeply involved in speculative research must not lose interest in practice, and it is useful for the progress of science to spread its results. For Halbwachs, the statistical method is only a routine for the one who cannot catch its spirit and its deep scientific meaning. Halbwachs, though he had been a student in literature, had extensively reflected on the meaning of statistical process in a study devoted to social life, in particular in his book on Quetelet published at the eve of the war. The idea of Fréchet and Halbwachs' book is to present the principles of probability and their application using only the most basic notions of mathematics.

It is therefore interesting to observe that Fréchet and Halbwachs published a book on probability as a result of their teaching of statistics at Strasbourg institute. Fréchet accepted from the beginning that mathematical statistics was generated by probability, as he claimed later in his 1948 address ([52]). Even if he began to concentrate on statistics during his Strasbourg period, Fréchet was essentially concerned by probability theory before the 1930s<sup>35</sup>. His statistical activity is therefore mostly situated after the period of time we consider in the present paper, as is proven by

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33. Vous savez mieux que personne l'importance considérable que les allemands avaient donnée à cette université et la coquetterie qu'ils ont mise à en faire une des plus brillantes sinon la plus brillante de l'empire. Vous avez certainement vu aussi qu'ils ont prédit en partant qu'en moins de 3 ans la France aurait saboté leur œuvre. Comment relever ce défi ?

34. J'ai recruté des Alsaciens -Lorrains de bonne souche. Je leur ai mis des types à côté. J'ai été voir les Allemands et je leur ai dit : je vous ai mis des Alsaciens-Lorrains à côté de vous pour que vous les mettiez entièrement au courant ce que vous avez à faire. Vous ne partirez que lorsque ces gens me diront qu'ils connaissent le métier. Alors, comme ils voulaient s'en aller, ça s'est bien passé.

35. See [66] for details on Fréchet's beginnings on probability and in particular his correspondence with Hostinský about Markov chains.



his late election at the head of the SSP at the venerable age of 70. On Fréchet's statistical works, consult [4] and [7].

**2.2. Teaching statistics. The ISUP.** Borel was not insensitive to Strasbourg's innovations. He used his new political and scientific influence (he had been elected to the Academy of Sciences in 1921) to help promote the teaching of mathematical statistics in Paris. As a result the *Institut de Statistique de l'Université de Paris* (ISUP - Statistical Institute of Paris University) was created in 1922.

Emile Borel convinced Lucien March and Fernand Faure from the Faculty of Law to join him in setting up the new institution. The Institute depended on the four faculties of Science, Medicine, Law and Literature. It was seen as a promising example of an interdisciplinary place for teaching. Such an institute perfectly illustrated the idea of knowledge, exchange and meeting - especially in the field of probability and statistics - that Borel sought to consolidate in France. The creation of the ISUP remained a well-kept secret, though it was mentioned for example in the journal *Vient de paraître* in January 1923, where a particular emphasis was put on the innovation it represented.

Paris University has just created a Statistical Institute, where the Faculty of Law and the Faculty of Science will collaborate ; its head office will be in the buildings of the Faculty of Law. At the same time, the construction of the Institute of Geography on rue Pierre Curie, where the Faculty of Science and the Faculty of Literature will fraternize, is being completed. When numerous institutes of that kind are truly alive, the word University will no more be only a word, meaning the reunion of several Faculties ignoring each other. Several Universities outside Paris have been for a long time, on that point as well as on others, ahead of the University of Paris<sup>36</sup>.

In his paper [91] Meusnier observed that at the ISUP the students were trained in four main subjects : demography and economy, actuarial sciences, industrial technique and research, and medicine. The aim of the institute was naturally to teach statistics, with officially both a theoretical and practical point of view. Nevertheless, the program during the first years of its existence showed a clear desire to insist on applications. The latter concern a wide range of domains such as finance, political economy, demography, biometrics, public health, prediction, insurance, trading, agriculture, transports, bank, credit or public finances.

Although the ISUP officially opened in 1922, the full program really began in 1924-1925. In the two previous years, there had been some lectures, but no diplomas were delivered. At the beginning, Borel taught the course of *statistical methods*, which dealt with mathematical statistics. The number of students at the ISUP during the first years remained extremely low. Only 4 students were registered in the academic year 1924-25, when the first diploma was delivered at the Institute. The fact probably fostered the skepticism with which the creation of the Institute was met by field statisticians - a skepticism obvious in the already mentioned interview of Henri Bunle [46] ; though the causality could also be in the other direction.

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36. L'Université de Paris vient de créer un Institut de Statistique, où collaboreront la Faculté de Droit et la Faculté des Sciences ; son siège sera dans les bâtiments de la Faculté de Droit. En même temps, s'achèvent rue Pierre Curie, les bâtiments de l'Institut de Géographie où fraterniseront la Faculté des Sciences et la Faculté des lettres. Lorsque de nombreux Instituts de ce genre seront bien vivants, le mot d'Université ne sera plus un simple mot, désignant la réunion de plusieurs Facultés s'ignorant les unes les autres. Certaines Universités des départements ont, sur ce point comme sur d'autres, devancé depuis longtemps l'Université de Paris.

With M.Borel, M.March has created the statistical whatchamacallit of the Sorbonne. M.Borel read a small course during one year, and then M.March, and then M.Huber on demography. When Borel was fed up, he passed on his course to Darmois who was in Nancy. Darmois began to collect information about what had been done in the United Kingdom. Because in the United Kingdom, they had been working more. There was a book for teaching statistics. He began to teach statistics. Besides, there is a book by him. He developed the mathematical point of view a bit more later. This was the ISUP regime<sup>37</sup>.

As Bunle mentioned, already in 1924 Borel asked Darmois to replace him.

**2.3. Darmois.** George Darmois was born on 24 June 1888 in Eply near Nancy. In 1906, he entered the École Normale Supérieure. Under Darboux's influence, in 1911 he began a doctorate on a subject mixing geometry and analysis. It concerned the study of partial differential equations arising from geometry. The title of his thesis was *About algebraic curves with constant torsion*. However, due to the First World War, Darmois defended his thesis only on 26 February 1921. Nevertheless, the War had other, more important influences on his scientific evolution. During the War, Darmois had worked on two military problems, one about sound ranging and the other about ballistics. He described how these studies had played a decisive role ([42]) :

The war of 1914-1918, having oriented me towards ballistics and shooting problems, and then towards location by sound and the problems of measuring and of wave propagation, had deeply inflected my spirit towards mathematical physics and the calculus of probability<sup>38</sup>.

After the war, Darmois became a professor of analysis in Nancy's University of Sciences. During the 1920s, Darmois' primary interest was mathematical physics, especially the theory of relativity. He published several articles about relativity, and as lately as 1930, he made a conference where he spoke about the experimental verifications of this theory and published a book in 1932 on the subject [39]. The peculiar destiny of relativity theory in France during the 1920s was studied in [101] through Eyraud's case. Eyraud was another young mathematician who subsequently turned towards statistics and founded a statistical institute in Lyon in 1930, the *Institut de Science Financière et d'Assurances* (Institute for Financial Science and Insurances), the first French institution to deliver a diploma for actuaries. In fact, Darmois was interested in any experimental aspect of science, not only in physics. In 1923, he was already teaching probabilities and applications to statistics. Darmois later explained ([42])

The decision I took in Nancy in 1923 to connect the teaching and investigations of probability calculus with several applications to statistics stemmed from the desire of constituting in France a school of theoretical and practical statistics.

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37. Avec Monsieur Borel, M.March a créé le machin de Statistique de la Sorbonne. Monsieur Borel a fait un petit cours pendant une année, et puis M.March, puis M. Huber sur la démographie. Quand Borel en a eu assez, il a passé son cours à Darmois qui était à Nancy. Darmois a commencé à se mettre au courant de ce qui avait commencé à se faire en Angleterre. Parce qu'en Angleterre, ils avaient travaillé davantage. Il y avait un volume pour l'enseignement de la Statistique. Il a commencé à enseigner la Statistique. D'ailleurs, il y a un volume de lui. Il a développé un peu plus du point de vue mathématique. Voilà le régime de l'ISUP.

38. La guerre de 1914-1918, en m'orientant vers la balistique et les problèmes de tir, puis vers le repérage par le son et les problèmes de mesure et de propagation des ondes, a très fortement infléchi mon esprit vers la Physique mathématique et le Calcul des probabilités

Great Britain and the United States showed the way, and it was important to follow their examples. I was thus led to participate in national, and later international, statistical activities<sup>39</sup>.

Apart from Darmais' intellectual interests for statistics, another reason why Borel called Darmais to Paris may have been the ties he had with the industrial world. In a recently published interview, Guilbaud informs us ([9]) that Darmais owned a small forge in the Vosges and frequently mocked his colleagues who disdained contact with industrialists.

In 1928, at the International Congress of Mathematicians in Bologna, Darmais gave a talk entitled *About the analysis and comparison of statistical series which could be developed in time (the time correlation problem)*. As Danjon wrote ([36])

Georges Darmais set himself two tasks. Firstly, to disseminate the power of statistical methods applied to sciences of observation, to biometrics, to applied psychology, to econometrics, to production control, to operational research, and so on. The astronomers cannot forget that he was the first in France to teach statistics and stellar dynamics as early as 1928-29. His apostolic mission for statistics was pursued relentlessly in the form of lectures, conferences, seminars. In some domains, success was instantaneous ; George Darmais' ideas, by dint of obstinacy, managed to permeate the least prepared environments and eventually made them prevail against routine. Moreover, he tried to improve those parts of the theory which seemed to deserve special attention. And so he especially cared about the general theories of estimation from random sampling, of which the general theory of errors is a particular case<sup>40</sup>.

In 1925-1926, the ISUP syllabus was clearly oriented towards applied statistics and the probabilistic content was rather elementary and superficial. Meusnier ([91]) listed the different items of the syllabus, which appear more as a collection of independent tools than as a real continuous exposition of a theory. It is worth noticing that the definition and *fundamental principles* of probability appear only as the 7th item of the syllabus. Moreover, the only theorem of probability theory mentioned is the (weak) law of large numbers.

At the ISUP, Darmais' lectures evolved with time. As we have seen, March also taught at the ISUP. March had been in 1912 the main initiator of Pearson in France. He translated *The Grammar of Science* into French and the book was published in Paris by Félix Alcan ([94]). He was probably a very useful source of information on biometry for Darmais. Darmais' progressive acquaintance with the tools of the foreign statistical schools (British, American, Scandinavian

39. La décision que j'ai prise à Nancy vers 1923 de joindre aux enseignements et recherches sur le calcul des probabilités diverses applications à la statistique, est venue du désir de constituer en France une école de Statistique théorique et pratique. La Grande-Bretagne et les États-Unis montraient le chemin, il importait de suivre leur exemple. J'ai ainsi été amené à prendre part aux activités statistiques nationales, puis internationales.

40. George Darmais s'était assigné deux tâches. En premier lieu, faire connaître la puissance des méthodes statistiques appliquées aux Sciences d'observation, à la Biométrie, à la Psychologie Appliquée, à l'Économétrie, au contrôle des entreprises, à la Recherche opérationnelle, etc. Les astronomes ne peuvent oublier qu'il fut le premier en France à professer la Statistique et la Dynamique stellaire, dès 1928-1929. Son apostolat en faveur de la Statistique s'est poursuivi sans relâche, sous la forme de cours, de conférences, de séminaires. Dans certains domaines, le succès en fut immédiat ; l'obstination de Geroge Darmais devait faire pénétrer ses idées dans les milieux les moins bien préparés et finalement les faire prévaloir contre la routine. Il s'est efforcé, en outre, de faire progresser les parties de la théorie qui lui paraissaient mériter une attention spéciale. C'est ainsi qu'il s'est particulièrement appliqué aux théories générales de l'estimation sur échantillon aléatoire, dont la théorie générale des erreurs est un cas particulier.

and so on) convinced him of the necessity of improving the probabilistic level of the lectures. This also perfectly fit Borel's ideas. This rapid evolution led to a reversal of priority between the statistical models and the probabilistic models. Darmon published the first French textbook on mathematical statistics entitled *Statistique Mathématique*. The book was published in 1928 by Doin with a preface by Huber. It shows a strong probabilistic orientation. Darmon eventually obtained a tenured position at Paris University only in 1933. In 1934, he published a work of popularization about statistics [40].

A thorough study of the ISUP syllabus is presented in [102], [99] or [92]. The comparison between the 1924 and the 1938 syllabi is instructive and gives a clear idea of the state of the statistical scene in France, after 20 years of effort. Now, a large amount of statistics was clearly presented as an emanation of probability theory. The mathematical level subsequently increased and the probabilistic tools introduced during the 1920s and the 1930s were extensively used. Probability distributions and limit theorems became omnipresent. Probability theory and its *main theorems* were introduced at the very beginning, as well as limit theorems and their tools, such as the *Chebyshev's method for the law of large numbers*. Moreover, a strong presence of analysis of correlation and regression shows that the mentioned notions had become basic knowledge. Furthermore, the number of students of the ISUP had slowly increased : for the academic year 1937-1938, there were 15 students.

**2.4. The Institut Henri Poincaré.** As we have seen, the ISUP was a place for teaching, not specifically devoted to mathematics. But in Borel's mind, probability and mathematical physics also needed a place to develop. That is why, thanks to the Rockefeller Foundation and Baron E. de Rothschild, in 1928, Borel managed to found an Institute dedicated to probability and mathematical physics, the *Institut Henri Poincaré* (Henri Poincaré Institute), in short the IHP. The purpose assigned to the Institute was in the first place to facilitate exchanges and meetings between specialists of these domains.

After the Great War, the Rockefeller Foundation offered credits - both for the victors and the (former) enemy - to develop scientific institutions in Europe through the International Education Board, its organization devoted to this kind of funding. This action not only helped developing new institutions but also saved scientific institutions threatened by the consequences of the war. The foundation paid in particular special attention to the countries where there was a strong cultural academic tradition such as Germany where it originated Göttingen Institute for Physics. It also contributed to the development of the Institute for Theoretical Physics in Copenhagen (see [33]). Siegmund-Schultze, in his detailed study of the Rockefeller foundation [109], gives details about the creation of the IHP. It is interesting to observe that Borel had direct personal discussions on the subject with Augustus Trowbridge the head of the International Education Board in Paris. In his inaugural speech, on 17 November 1928, Borel presented a slightly nationalistic picture of the French origins of probability theory.

Probability calculus and mathematical physics are two sciences whose origin is French to a large extent. Should we mention for probability calculus : Fermat, Pascal, d'Alembert, Buffon, Laplace, Cournot, Joseph Bertrand, Henri Poincaré, and for mathematical physics : d'Alembert, Poisson, Fourier, Ampère, Cauchy and again Henri Poincaré ? It is only in the second half of the 19th Century that progress in science led us to understand the tight links that exist between both sciences which look distinct at first sight, and to realize that the properties of

matter and energy studied by mathematical physics are subject to probability and statistical laws<sup>41</sup>.

In the same speech, Borel described the purpose of the Institute, naming scientists who were supposed to come for talks or lectures at the IHP. He mentioned in particular Einstein and Volterra.

Other first-rank scientists have let us hope they will participate. Thanks to them, the Institut Henri Poincaré will really be international, not only by virtue of the students who will attend its lectures, but also because of the professors who will give talks and lectures there. It will not only contribute to the progress of science, but also will act as a bridge between peoples by letting scientists from every country collaborate, meet and better understand each other<sup>42</sup>.

The opening of the IHP allowed Borel to create some new courses in Paris. Borel's lectures on probability (entitled *Probability theory and its application*) were given at the IHP. Other lectures on the subject were given by Maurice Fréchet whom Borel had managed to call from Strasbourg as a professor *without chair* ('sans chaire') at the Sorbonne, with the obvious objective of being in charge of probabilities in the new Institute. The lectures Fréchet gave were entitled *The law of large numbers* and *The theory of integral equations*. As was said in Borel's inaugural speech, foreign lecturers were invited every year as well to give talks about their field of interest and investigation.

The main subject taught at the Institute was however physics, as well as several topics of *applied mathematics*. Darmais was asked by Borel to give lectures at the *Institut* on statistics from its very opening, in 1928, when he was still a professor in Nancy. An announcement from academic year 1928-29 asserts that *M.G.Darmais, professor at the University of Nancy, will give a series of four lectures on the following topic : Statistical laws, correlation and covariance with applications to heredity, to social and economic sciences*. The lectures took place in April 1929. During the first year, there was no mention in the syllabus of other lectures related to statistics. Until he obtained an academic position in Paris, Darmais would come every year to give some talks and lectures at the IHP and he would remain the only speaker on statistics. The list of invited foreign speakers clearly emphasizes that scientists dealing with physical questions were mainly invited. During the first two years, Fermi, Einstein, Dirac or also Sommerfeld were invited, to mention only these famous names. Among the first specialists in probability, we see Hostinský, Pólya, and also Paul Lévy. The part of probability progressively grew among the subjects of the conferences - and later statistics was also represented.

An important feature of the IHP was that Borel used the new institution to help his foreign colleagues in the grip of the European interwar convulsions. The administrative archives of the

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41. Le calcul des probabilités et la physique mathématique sont deux sciences dont l'origine est pour une grande partie française. Faut-il nommer pour le calcul des probabilités : Fermat, Pascal, d'Alembert, Buffon, Laplace, Cournot, Joseph Bertrand, Henri Poincaré, et pour la physique mathématique : d'Alembert, Poisson, Fourier, Ampère, Cauchy et encore Henri Poincaré ? C'est seulement dans la seconde moitié du XIX<sup>ème</sup> siècle que le progrès de la science amena à comprendre les liens étroits qui existent entre les deux sciences au premier abord distinctes et à se rendre compte que les propriétés de la matière et de l'énergie qu'étudie la physique mathématique, sont soumises à des lois de probabilité, à des lois statistiques.

42. D'autres savants de premier ordre ont laissé espérer leur concours. Grâce à eux, l'Institut Henri Poincaré sera véritablement international, non seulement par les élèves qui suivront son enseignement mais aussi par les professeurs qui y donneront des cours et conférences. Il ne contribuera pas seulement au progrès de la science, mais au rapprochement des peuples en permettant à des savants de tous les pays de collaborer, de se connaître et de mieux se comprendre.



Institute contain several interesting documents in that respect. For example a letter from the minister for public instruction to Borel (via Dean Maurain) in 1932, mentioning that Volterra had been *disbarred from university staff for political reasons and left for Paris*. After Hitler's nomination as Chancellor in Germany in 1933, an increasing number of German refugees were invited to give talks at the IHP<sup>43</sup>. The ones Borel never succeeded to invite were the Soviet scientists confronted with closed borders after Stalin's power consolidated. Whereas Moscow was in the 1930s the indisputable world center of the mathematics of randomness, it is rather amazing not to see a single Soviet name among the speakers. The presence on the list of mathematician Vladimir Alexandrovitch Kostitsyn was only due to the fact that he managed to come to Paris in 1928 under the pretext of medical emergency and never returned to the USSR.

As we have already mentioned, apart from Darmois, no statistician was invited during the first three years of existence of the IHP. Only in 1931 did a first foreign statistician give a talk. Subsequently in the 1930s most statistical trends were represented in these conferences : the Scandinavian (Guldborg, Steffensen), the British (Neyman, Fisher), the German (Gumbel) in particular. This was the result of a kind of deliberate strategy for importing foreign statistical technique to France. We shall now deal with this question.

### 3. TECHNOLOGY TRANSFER

We have already evoked in the previous section how France lagged behind on statistical methods when compared to other countries. We shall try to emphasize the role played by Borel to improve this situation - not the least via the creation of the IHP.

It seems there was a convergence of view between Borel, Darmois and Fréchet concerning the statisticians whose works had to be presented at the IHP. In 1933 a volume of Borel's great *Treaty of calculus of probability and its application* was published by Risser and Traynard ([100]) - the only one whose title included the words *Statistique Mathématique*. Most of the material of the conferences on statistics at the IHP during those years were included in the book. The American mathematician Edward Lewis Dodd, whom Aldrich claim to be one of the most influent American statisticians of the time ([1]), wrote a review for the American Mathematical Society where he underlined the up-to-date treatment of mathematical statistics by Risser and Traynard ([48]).

**3.1. The Annales.** To facilitate the diffusion of the written text of the conferences, the *Annales de l'IHP : Recueil de Conférences et Mémoires de Calcul des Probabilités et Physique Théorique* (Annals of IHP : A collection of Conferences and Memoirs on Calculus of Probability and Theoretical Physics) were founded in 1930. Contrary to the current publication bearing the title *Annales de l'IHP*, the *Annales* in the 1930s were not really conceived as a journal but as a publication devoted to recording the lectures held at the Institute. For many years, there were no spontaneous submissions of papers. Our efforts to find announcements of this publication abroad has produced only slight results, which nonetheless reveal at least a limited diffusion. The only true advertisement we noted was in the Bulletin of the AMS in 1931 ([73]). It reads

A new journal entitled Annales de l'Institut Henri Poincaré, Recueil de Conférences et Mémoires de Calcul des Probabilités et Physique Théorique has been founded by the Institut Henri Poincaré for the publications of lectures delivered by invitation at that institute. The first issue contains articles by C.G. Darwin, A. Einstein, and E. Fermi.

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43. Among them, Gumbel settled down in France - see [68].

Until 1964, there was only one series of the *Annales*, mixing papers on physics or mathematical analysis with others on probability or statistics. In fact, a majority of papers were devoted to physics. The list of publications on probability and statistics reveals 17 articles before World War 2.

With the exception of Paul Lévy (maybe because his talks were organized by oral agreement), we have been able to locate the moment when the lectures were given. There was generally an average two years' delay between the lecture and its publication, and this probably resulted in a final form slightly more fully worked out than the oral lecture. As the lectures were really an opportunity for the speaker to present research in progress, the later publication was enriched with more recent results and discussion. Through the politically-correct tone of the academic prose, one may sometimes feel several scientific polemics of the time such as the fiery discussions around the foundations of probability theory (von Mises was invited in 1931, Cantelli in 1932, de Finetti in 1935, Reichenbach in 1937). Or one can also follow the evolution of a new subject such as the theory of Markov chains. Hostinský's 1937 paper, made after his lectures the same year, testifies to the predominance of the Soviet school of probability with Kolmogorov as a beacon, and also refers to Doeblin's work (see [85]).

Year	Vol.	Pages	Author	Title	Date of conferences	Nationality
1930	1	117-161	G. Pólya	Sur quelques points de la théorie des probabilités	March 1929	Hungary
1930	1	163-175	Paul Lévy	Le théorème fondamental de la théorie des erreurs	?	France
1932	3	1-74	B. Hostinský	Application du Calcul des Probabilités à la Théorie du mouvement Brownien	January-February 1930	Czechoslovakia
1932	3	137-190	R. von Mises	Théorie des probabilités. Fondements et applications	November 1931	Germany
1932	3	191-228	G. Darmais	La méthode statistique dans les sciences d'observation	1929	France
1933	3	229-276	A. Guldberg	Les fonctions de fréquence discontinues et les séries statistiques	April 1932	Norway
1933	3	319-344	J.F. Steffensen	Deux problèmes de Calcul des Probabilités	December 1931	Danemark
1933	3	465-490	G. Castelnuovo	Sur quelques problèmes se rattachant au Calcul des Probabilités	April 1932	Italy
1935	5	3-50	F.P. Cantelli	Considérations sur la convergence dans le Calcul des probabilités	May 1932	Italy
1935	5	115-158	E.J. Gumbel	Les valeurs extrêmes des distributions statistiques	December-January 1933-1934	Germany
1935	5	159-176	A. Guldberg	Sur les lois de probabilités et la corrélation	April 1934	Norway
1936	6	153-184	P. Lévy	Sur quelques points de la théorie des probabilités dénombrables	?	France
1936	6	185-212	R. de Mises	Les lois de probabilités pour les fonctions statistiques	1934-1935	Germany
1937	7	117-161	B. de Finetti	La prévision : ses lois logiques, ses sources subjectives	May 1935	Italy
1937	7	163-175	B. Hostinský	Sur les probabilités relatives aux variables aléatoires liées entre elles. Applications diverses	January 1937	Czechoslovakia
1937	7	1-74	H. Reichenbach	Les fondements logiques du calcul des probabilités	May-June 1937	Germany
1938	8	137-190	F. Tricomi	Les transformations de Fourier, Laplace, Gauss, et leurs applications au calcul des probabilités et à la statistique	1937-1938	Italy

Papers about probability and statistics  
in *Annales de l'Institut Henri Poincaré* during the 1930s

All this draws a picture of the IHP as a place with lively exchanges in the study of mathematics of randomness during the 1930s. The collection of the 17 papers of the *Annales* reflects the efforts of the time in that direction - with the noticeable and already mentioned absence of Soviet authors. A certain prevalence of theory over application can be observed - which results in an orientation which favors probability over statistics. This is not surprising, as we said in the previous section that in Fréchet and Darmais' vision of the mathematics of randomness, probability theory was the basis on which to build statistics. More significantly, we observe that important foreign traditions are represented in this list : Scandinavian (Guldberg, Steffensen), British (Neyman), German (Gumbel) ; as we shall see below, Darmais' main purpose in his 1929 lectures (the first lectures in statistics at the IHP) was to present general methods obtained by Pearson's biometric school in Britain. In fact, a careful observation of the statistical methods exposed at the Institute reveals significant choices. We have mentioned that Borel had known about Pearson's methods early in

his scientific life, maybe through Volterra who had always advocated them (see [119] and [120]). But we have seen that Borel had also expressed deep criticisms towards other aspects of the British studies of randomness, when he commented on Keynes' treatise.

Did Borel's distrust for British *different minds* exert an influence on the choices? R.A. Fisher, for example, who became Pearson's successor as the central figure of British statistics during the 1930s, was invited only in 1938 and his lecture was not published. Nor does it seem that his methods were presented at the Institute by other scientists<sup>44</sup>.

As an illustration of the technology transfer in statistics sought by the IHP team, we shall now concentrate on the first two papers published in the *Annales* devoted to statistical topics. These two papers, written by Darmon and Alf Guldberg, were the occasion for presenting results of Pearson's school, and of the Scandinavian tendency.

**3.2. Darmon's paper.** Darmon took time before finalizing the manuscript of the 1929 lectures mentioned above. The published version [38], which appeared in 1932, indicates that it was received on 25 July 1931.

Rather than a paper discussing a precise theoretical point, Darmon presented a survey about series of observations, and he presented a collection of methods to detect what he called 'persistences' (in French *permanences*), that is to say laws, deterministic or not, which provide a theoretical framework for the model under study. The program is clearly set at the beginning of the paper.

The aim of these lectures is to emphasize several fundamental ideas of statistics and the character of the special order they can bring into some sets of experimental results.

A research project usually stems from the desire to verify, deepen and link a group of ideas, either because of its practical importance, or from its unquestionable presence.

As observation advances in the field under study, the accumulated results are thus placed either within the framework of a prior theory, sometimes vague, or in a purely pragmatic order, generally with some logical threads running through it.

This provisional organization, as a convenient description of the observed material, allows the mind to handle large sets more easily.

After that, one looks for what we may generally call persistences, which is to say relations commonly observed which constitute empirical laws for the observed phenomena. (...Once these empirical laws are obtained, the subsequent process of the mind aims at understanding them, explaining them, relating them.

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44. It is true that Darmon quotes Fisher twice in his paper (pp. 209 and 223) but it is only in passing, as a source of original applied situations. We note further that Edward L. Dodd, in his laudatory review about Risser and Traynard's book ([100]) did not seem to know Fisher's work either. It is only from 1935 on that Darmon grasped the epoch-making aspect of Fisher's techniques about estimation. Darmon's lectures at ISUP in 1937, edited by Pierre Thionet ([41]) show the high technical level attained in the exposition of notions such as sufficient statistics and hypothesis testing. Another explanation for Fisher's absence was that Borel, Darmon and Fréchet probably observed the intellectual wars raging in Great Britain between the supporters of and opponents to Bayesian inference and it does not seem that they wished the IHP to become a battlefield for these quarrels. On the complicated story of British statistics of that time (and especially on Keynes' intervention in the debate), see [72] and [2] and the references included.

In parallel with the descriptive order, the explanatory or logical orders, scientific theories and laws develop. This development, which is constructed from several basic notions, must then recover the results of the experiment<sup>45</sup>.

The paper can be roughly divided into four parts (which may correspond to the four lectures Darmois presented), separated by a three-star symbol.

The first section is devoted to a general reflection about the notion of statistical permanence. It is deprived of any mathematical apparatus and essentially seeks to illustrate the notion through classical examples such as Mendel's hybridization, or radioactivity. Darmois' examples emphasize the role of the urn model, where alternatives are represented by balls of different colors. Darmois insists on the fact that the urn model can be very useful as a tool even if it may not provide an explanatory model. For the example of radioactivity, he explains : *Everything happens as if there were the same probability of disintegration for each atom. . . In each atom, there is an urn from which the drawings are made* though, naturally, *the explanation seems quite intricate* - an intricacy that Darmois in fact does not absolutely reject due to the *new mechanics* (wave mechanics) for which he refers to Haas' 1928 book [64], translated into French in 1930 and prefaced by Borel. But this is not the point in Darmois' statistical presentation and, as he stipulates in his previous example of Mendelian hybridization, to represent the statistical persistences, one may work with the urn considered *as a fact*.

The second part of the paper is devoted to the presentation of several urn models, with increasing sophistication to render them more adapted to describe particular situations of '*one parameter persistences*' (*permanence à une variable*). Darmois illustrates the elementary case of how to obtain a confidence interval by means of the de Moivre theorem for a simple Bernoulli (independent and identically distributed) sample. Darmois emphasizes Pearson's observation that the aim is *to obtain an image of the statistical persistences from the theory of probability*. This implies that it cannot be sufficient to provide limit theorems to deal with a statistical problem. Only a clever use of these theorems can provide a model for the fluctuations or the stability. This insistence on the necessity of obtaining a good-quality approximation shows that what nowadays seems an obviously required part of the statistical treatment was at the time not so well realized. Darmois presents successively Poisson, Lexis, Borel, and Polya's urn schemes. Interestingly, he proposes concrete data which, he says, correctly fit these models. The examples chosen for Lexis and Polya's models are rather classically related to mortality or infection data, the kind of figures which invaded statistical studies in continental Europe as well as in Great Britain. However, when completing his list of models with considerations about the Gaussian distribution, Darmois refers the

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45. Le but de ces leçons est de mettre en évidence quelques idées essentielles de la statistique et le caractère de cet ordre spécial qu'elle peut mettre dans certains ensembles de résultats expérimentaux. Un ensemble de recherche se présente généralement, soit comme suite à un désir de vérifier, d'approfondir et lier un groupe d'idées, soit comme imposé par son importance pratique, par son indiscutable présence. A mesure que l'observation progresse dans le domaine qu'elle a choisi, les résultats qu'elle accumule sont alors placés, soit dans les cadres parfois assez vagues d'une théorie préalable, soit dans un ordre de pure commodité, généralement sillonné de quelques filaments logiques. Cette organisation provisoire, description commode du matériel observé, permet à l'esprit de dominer avec plus d'aisance des ensembles étendus. On cherche ensuite, ce que d'une manière générale, nous appellerons des permanences, certaines relations qui demeurent courantes, et qui constituent des lois empiriques des phénomènes observés.(...) Ces lois empiriques une fois obtenues, les démarches suivantes de l'esprit visent à les comprendre, les expliquer, les relier entre elles. Parallèlement à l'ordre descriptif se développent l'ordre explicatif, logiques, les théories et les lois scientifiques. Ce développement, qui se construit à partir de certaines notions à la base, doit alors retrouver les résultats de l'expérience



reader to recent economic works by Gibrat ([54])<sup>46</sup>. Desrosières ([45], pp.200-203) has already observed that before 1930, the use of statistical models by economists had not been frequent. Dar-mois, as well as sociologists Halbwachs and Simiand, were innovators through their contact with economists during the 1920s (such as Divisia<sup>47</sup> (1889-1964) - the same that introduced the word *permanence*, or Gibrat) with strong mathematical backgrounds who were sensitive to statistics. At the beginning of the 1930s, all these scholars met at the newly founded *Centre polytechnicien d'études économiques* devoted to the study of the Great Depression.

The third part of the paper is devoted to the general question of correlation. After a brief presentation of stochastic dependence and of elementary theory of regression (defining the correlation coefficient and linear regression), Dar-mois gives general results obtained by Galton and Pearson's biometric school. The main examples deal with hereditary phenomena where the question is to bring precision about the influence of ancestors on their descendants (the presence of a character, sex ratio at birth for examples). As Dar-mois writes : *The aim of the biometric school is to obtain connected distributions; in other words, given the value of a character in the father and the grandfather, to deduce the distribution of the same character in the sons with this very ancestry*<sup>48</sup> ([38], p.213).

The last and fourth part of Dar-mois' paper introduces time as a parameter and studies the specific kind of models which can be developed to face this situation. As Dar-mois explains, time was naturally present for example in the case of astronomic observations, but with *the particular chance that the motion of the earth and the planets around the sun produce simple and rather pure regularities due to the enormity of the mass of the sun. ( ... ) But those problems presented by social and economic sciences that statistical methods may seek to clarify and to solve, are very different*<sup>49</sup>. The evolution of a quantity is therefore presented in this fourth part as a model integrating random perturbations. Dar-mois presents several models of that kind, but mostly limits his presentation to description and commentaries, not even summarizing the corresponding technicalities. This limitation to a qualitative exposition is probably interpretable as another sign of the low level of statistical knowledge in France at the time, making it necessary to provide a general survey of existing situations before considering the mathematical treatment. Moreover, the four papers referred to in this part were recent publications from the Anglo-Saxon world, a trace of Dar-mois' efforts during the 1920s to get acquainted with British and American works in that direction.

The first model described by Dar-mois was considered by Udny Yule in his 1927 paper [118] in relation to the observations of sunspots. It concerned the presence of random disturbances of an oscillatory system which Yule illustrated by the image of a pendulum *left to itself, and unfortunately boys*<sup>50</sup> *got into the room and started pelting the pendulum with peas*. Yule proposed two methods to investigate the hidden periodicities of the phenomenon, but as we mentioned, Dar-mois did not go into such details. Dar-mois found another kind of model in Hotelling ([71]). Hotelling considered a differential equation whose coefficients contains a random perturbation.

46. About Gibrat, see [47] and [3]

47. On Divisia, see in particular [104][80].

48. Le but de l'école biométrique anglaise est en effet d'obtenir des répartitions liées, autrement dit, connaissant la valeur du caractère pour le père et pour le grand-père, d'en déduire la loi de répartition du même caractère pour les fils ayant cette ascendance fixée.

49. Avec cette chance particulière que le mouvement de la terre et des planètes autour du soleil fait apparaître des régularités simples et assez pures, dues à la masse énorme du soleil. ([38], p.218 )

50. not girls... The detail may be of importance !

Hotelling explained in his paper how it may be more appropriate to work with the coefficients of the equation rather than with the solution of the equation when a statistical verification of the model is needed. However, also here, Darmois limits himself to mere description. Other models are borrowed from Ronald Fisher and Egon S. Pearson's recent papers. In conclusion, Darmois' paper appears as a vast catalogue of situations opening the door to future mathematical studies, with a noticeable inclination towards economic phenomena.

**3.3. Guldberg's paper.** Let us now turn towards the second paper, written by Norwegian statistician Alf Guldberg (1866-1936).

Few documents are easily accessible on Guldberg. About his youth as a Lie student traveling in Europe, one may consult [115] chapter 24. Back in Norway in 1903, he obtained a position at Christiania<sup>51</sup> University and soon began to teach actuarial statistics. Guldberg was often mentioned in the Čuprow-Bortkiewicz correspondence ([108]) as an influential member of the Scandinavian mathematical scene.

At the beginning of the 20th Century, Scandinavia was a first-rank center for approximation techniques, not behind the British school (see [105]). Those techniques were inherited from the works of astronomers of the second half of 19th century, such as Thiele (1838-1910), Gylden (1841-1896) or Charlier (1862-1934)<sup>52</sup>. . . In a draft of an introduction for Steffensen, when he came to Paris in 1932 to give a talk at the IHP,<sup>53</sup> Fréchet declared

I have the great pleasure to introduce to you M.Steffensen, professor at the University of Copenhagen. It will be for us a new experience which, I am convinced, will be very valuable. Up to now, the lectures which have dealt with probability calculus have only considered it on the theoretical plane or through its application to physics. M.Steffensen, to whom the whole theoretical part of probability calculus is familiar, is now well known for the way he wonderfully had allowed the actuarial sciences to profit from mathematical advances. In a book about interpolation he systematically introduced a demand too often left aside : never to introduce an approximation without trying to specify the error. It is a relatively moderate advance in theoretical mathematics but it did not penetrate deeply into applications. Thanks to M.Steffensen no one can any longer ignore first that this estimation of error is necessary, and second, that thanks to him it is feasible.<sup>54</sup>

51. The name of Oslo before 1925.

52. See [86] for the filiation between the statistical approach of the continuous fraction development proposed by Gylden and Borel's theory of denumerable probabilities, and also [78] and [70] about astronomers in Scandinavia.

53. We found this draft by chance among the incredible mountain of documents of Fréchet's archives in the Paris Academy of Sciences.

54. J'ai un très grand plaisir à vous présenter M Steffensen professeur à l'Université de Copenhague. Ce sera pour nous une nouvelle expérience et une expérience qui j'en suis sûr sera très heureuse. Les conférences qui vous ont parlé jusqu'ici du calcul des probabilités ne le montrent que du point de vue théorique ou du point de vue des applications aux Sciences Physiques. M. Steffensen à qui toute la partie théorique du calcul des probabilités est familière, s'est fait connaître par la façon magistrale dont il a su faire profiter les sciences actuariels des progrès mathématiques. C'est ainsi que dans un livre sur l'interpolation il a introduit systématiquement une exigence trop souvent laissée de côté : celle qui consiste à n'introduire aucune forme approchée sans chercher à en chiffrer l'erreur. C'est là un progrès relativement modéré accompli dans la mathématique théorique mais qui n'avait pas pénétré profondément ses applications. Grâce à M. Steffensen on ne peut plus ignorer d'abord que cette estimation de l'erreur est nécessaire, ensuite que grâce à lui elle est possible.

Steffensen's book referred to by Fréchet, *Interpolationslære* was published in Danish in 1925, and an American translation ([112]) rapidly became available, appearing in 1927. In the Bulletin of the AMS [113], the notice explained what Steffensen's book contains.

Professor Steffensen's book is the outgrowth of the lectures which the author has given to actuarial students at the University of Copenhagen and is, with a few additions and simplifications, a translation of the Danish edition published in 1925. The book is intended as a text for students in American colleges and requires as mathematical equipment only an elementary knowledge of the differential and integral calculus. In a few places where the gamma function has been used the paragraphs have been printed in smaller type and may be omitted without breaking the continuity of the text. The topics covered are (1) the general theory of interpolation and extrapolation including the standard formulas usually associated with the names of Newton, Gauss, Stirling, Bessel, and others; (2) numerical differentiation; (3) numerical integration; (4) numerical solution of differential equations. Professor Steffensen's treatment is more rigorous than is usual in books on interpolation. This is important not merely from the point of view of the pure mathematician but also because of the increased number of formulas with workable remainder terms. It should not be supposed, however, that this adds to the difficulty of reading the text. The style is clear and, after the meaning of the symbols has been mastered, the book should prove very valuable to the increasing number of Americans who require some knowledge of this field of mathematics. The formulas and methods are illustrated by simple numerical examples, but the value of the book for class room use would be increased if it contained some problems to be solved by the student.

The mathematical study of interpolation was Steffensen's known speciality, as Čuprov in Prague wrote to Bortkiewicz<sup>55</sup>: *Steffensen's new textbook is once again written in Danish. It contains considerations on interpolation, which is Steffensen's speciality.* The American version remain the only translation of the book. In France, it seems to have been unknown for quite a while. More important for Guldberg's lectures in Paris was another aspect of Scandinavian statistics. Thiele had defined in the 1880s a mathematical object, the *halfinvariants*, later rediscovered by Fisher under the name *cumulative moment functions* and finally *cumulants*, as they are called today. We shall comment on this notion later.

As we mentioned above when we described Darmon's paper, in the 1930s, the British statistical school was predominant in Western Europe and Guldberg's presence in Paris may have been a reaction to advertise the methods of the Scandinavian school. Two points could corroborate this hypothesis. Fisher's paper - where he introduced cumulants - was published in 1929 [51], and there was no reference to Thiele in it. Fisher was later informed about Thiele's priority. He nevertheless did not entirely accept the fact and constantly underrated Thiele's treatment of halfinvariants<sup>56</sup>. In Guldberg's paper ([62]), there is clearly a desire to display Scandinavian works, and Thiele's in particular. More generally, Guldberg seems to have engaged in frenetic activity on the international scene to obtain recognition of Nordic mathematics. In particular, he was the main artisan of the Oslo International Congress of Mathematicians in July 1936, according

55. See [108], letter dated 2 April 1925.

56. See details in Lauritzen [78], or Hald [65] (pp.344-349).

to Carl Størmer, president of the congress, who replaced Guldberg at the last moment due to the latter's sudden death in February.

It was during the last international congress in Zürich that the Norwegian mathematicians took the decision to propose that the next congress could be held in Oslo<sup>57</sup>. It is our colleague, the regretted Alf Guldberg, who took this initiative, and who since then had constantly worked to guarantee the success of the congress, and above all to place it on a reliable economic basis. We had hoped that Guldberg could open the Congress as president. But, as every member of the congress knows well, a premature death snatched him away from his work. Nobody but we, who must finish his work, can better realize what a loss it was for the Congress to have been deprived of our eminent friend, who was so efficient in attracting friends and knew the art of association with his fellows (*sic*)<sup>58</sup>.

Let us briefly introduce Thiele's halfinvariants. Thiele first presented the halfinvariants in his 1889 book *Almindelig Iagttagelseslære : Sandsynlighedsregning og mindste Kvadraters Methode* (The general theory of observation : Calculus of probability and the method of least squares), translated and edited in [78].

If we denote by  $\sigma_n$  the  $n$ -th moment of a random variable, the halfinvariants  $\mu_n$  were first defined by the following recursion formula

$$\sigma_{k+1} = \sum_{i=0}^k \binom{k}{i} \sigma_{k-i} \mu_{i+1}$$

Moreover, Thiele explained in his book that the first four halfinvariants were the most important ones. The first was equal to the mean, the second to the variance, the third measured the *skewness* and the fourth the flatness of a distribution. For Hald ([65], p.209) there are basically two important questions.

- (1) How did Thiele find the recursion formula defining the cumulants ?
- (2) Why did he prefer the cumulants over the central moment ?

In his book of 1889, Thiele explained that the halfinvariants are often small.

We shall later become acquainted with certain favourable properties that distinguish the halfinvariants from the reduced sums of powers ; here we only mention that the halfinvariants most often are numerically smaller than the others. Whereas the reduced sums of powers of even degree are always positive, all halfinvariants except  $\mu_2$  may be negative just as well as positive, and therefore they will often have values that are not far from zero. ([78], pp. 84-85)

57. In [123], the report of the final session mentions that indeed Guldberg made the suggestion which was acclaimed by the Congress.

58. Ce fut au dernier congrès international de Zurich que les mathématiciens norvégiens résolurent d'émettre le projet que le prochain congrès se réunirait à Oslo. C'est notre collègue, le regretté Alf Guldberg, qui en prit l'initiative, et qui depuis s'est préoccupé inlassablement à assurer le succès du congrès, et surtout à l'établir sur une base économique solide. Nous avions espéré que Guldberg pourrait ouvrir le Congrès comme président. Mais, comme le savent tous les membres du congrès, une mort prématurée vint l'arracher à son oeuvre. Personne mieux que nous qui avons dû achever son travail, ne peut savoir quelle perte ce fut pour le congrès que d'être contraint à se passer de notre éminent ami, qui savait si bien gagner des amis et connaissait l'art de fréquenter ses semblables. ([93])

Only ten years later, in paper [116] did he realize that the halfinvariants could be directly expressed as coefficients of the Taylor expansion of the Laplace transform's logarithm.

$$\exp\left(\sum_{i=1}^{\infty} \frac{\mu_i t^i}{i!}\right) = \int e^{tx} f(x) dx.$$

In [116], Thiele proved that the halfinvariants for the Gaussian distribution possess an important property : only the first two cumulants are non equal to zero. This could have been another fundamental reason to use them.

Guldberg went to the IHP in April 1932 for the first time. He returned later to the IHP in April 1934. His two talks were published in the *Annales* as [62] and [63]. Both papers mainly deal with the same statistical technique, namely the application of Thiele's halfinvariants to the identification of an unknown probability distribution : in [62] for a single random variable, in [63] for two. Let us now comment on [62], which was preceded in 1931 by a Note to the *Comptes-Rendus* ([61]), presented by Borel.

The article is divided into four parts. In the first one, Guldberg introduces definitions and notations, and applies them to some examples. In particular, he introduces Thiele's halfinvariants, giving both definitions successively provided by Thiele as we mentioned above.

The reason why Thiele introduced the halfinvariants, a notion which seems rather artificial, is the following : if the observations follow a Gaussian distribution, every halfinvariant of order  $r > 2$  is equal to 0. As one of Thiele's former students, my colleague M.Heegard, made me observe, one has therefore a way for examining if a statistical collection of data can be represented by a Gaussian distribution or not.

Halfinvariants also have other advantages. When one linearly transforms the observations

$$x_i = ax_i + b$$

that is to say, when one changes the origin of the observations and the unity which measures them, the halfinvariants are changed according to the following formulae

$$\begin{aligned}\mu'_1 &= a\mu_1 + b \\ \mu'_r &= a^r \mu_r, r > 1\end{aligned}$$

(...) The mean moments are transformed in a more complicated way.

These relations show the great importance of halfinvariants for the study of a statistical collection of data<sup>59</sup>.

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59. La raison pour laquelle THIELE a introduit les semi-invariants, notion qui semble quelque peu artificielle, est la suivante : si les observations suivent une loi de GAUSS, tous les semi-invariants d'ordre  $r > 2$  sont nuls. On a donc, comme me l'a d'ailleurs fait remarquer un ancien élève de THIELE, mon collègue M. HEEGAARD, un moyen d'examiner si un ensemble statistiques peut se représenter par l'intermédiaire de la loi de Gauss ou non. Les semi-invariants ont cependant d'autres avantages. Quand on fait une transformation linéaire des observations

$$x_i = ax_i + b$$

c'est-à-dire, quand on change l'origine des observations et l'unité qui les mesure, les semi-invariants se transforment d'après les formules

$$\begin{aligned}\mu'_1 &= a\mu_1 + b \\ \mu'_r &= a^r \mu_r, r > 1\end{aligned}$$



In the second part of his article, Guldberg presents how Pearson represented statistical series, and criticizes the 'British method' because *M. Pearson's curves* are reasonably suitable only in the case when one *a priori* knows that the data can be represented by one of these curves<sup>60</sup>. Guldberg therefore opposes what he calls the *continental method* to the methods of the British biometric school. Let us observe that the continental statisticians that Guldberg named are almost only Scandinavian : Gram, Thiele, Bruns, Charlier, to which only Čuprov and Bortkiewicz were added<sup>61</sup>.

In fact, the real purpose of the paper is presented later, when Guldberg write that he *will try to study these questions in another way*. He first lists the problems, and then tries to answer to them.

The theory of functions of frequencies presents four important problems :

- The numerical computation of the function
- How to proceed if one wants to substitute a continuous function for the discontinuous one, which takes precisely the same value as the latter for integer values of the variable
- The determination of the moments of a given function of frequencies
- A statistical series being given, look for a function of frequencies which gives an approximate representation of it and specify, if possible, the necessary and sufficient criteria for a definite function to fulfill the required conditions<sup>62</sup>

The first and the second problems correspond to Guldberg's desire to interpolate the (discrete) empirical frequency function by a *continuous* (in the sense of *regular* : he asks for derivatives to exist) one, in order to use approximation techniques provided by real analysis. The meaning of the third point is clear : it is necessary to compute empirical and theoretical halfinvariants in order to compare them. The fourth point is the general method Guldberg wants to present for the statistical inference of a series of data, a method which relies on halfinvariants. After this general presentation, Guldberg looks at these four questions in particular cases for the theoretical distribution. For each theoretical distribution he considers (Poisson, Binomial, Pascal and hypergeometric), he provides an analytical expression  $\alpha(k)$  depending on the halfinvariants and which is constant. The method corresponding to the last point of his program is then to check whether this function on the empirical halfinvariants is constant. However he only illustrates his full program for the Poisson distribution ([62], p.235).

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(...) Les moments moyens se transforment d'une manière plus compliquée. Ces relations montrent la grande importance que présentent les semi-invariants pour l'étude d'un ensemble statistique

60. [62], p.246

61. Both kept tight contact with the Scandinavian group during the 1920s as attested by their correspondence ([108]).

62. La théorie des fonctions de fréquence pose quatre problèmes assez importants à savoir :

- Le calcul numérique de la fonction
- La manière de procéder si l'on veut substituer à la fonction discontinue une fonction continue, qui pour des valeurs entières de la variable prenne justement les même valeurs que la première.
- La détermination des moments d'une fonction de fréquence donnée[...].
- Une série statistique étant donnée, chercher une fonction de fréquence qui en donne une représentation approchée et établir, s'il est possible, les critères nécessaires et suffisants pour qu'une fonction déterminée remplisse les conditions requises.

## CONCLUSION

Whereas mathematical statistics based on probability theory was nearly absent from the French academic scene before the Great War, it had become, after World War 2, a topic well established in France. And it was not only the institutions created in the interwar period (ISUP in Paris, other various institutes in the provinces - such as the IEC - Institute for Commercial studies - in Strasbourg or ISFA (Institute of Financial Science and Insurance) in Lyon - and the IHP for the theoretical aspects. . .) carried on activities which included this large part of the mathematical sciences ; other important structures were founded later, such as INSEE or ENSAE, to facilitate the teaching of specialized sides of those techniques. As we have seen, it was under the influence of Borel that this field of mathematics began to mature in the 1920s. Borel's activity mixed political and social engagements with his mathematical interests, for example through the creation of the journal *Revue du mois* he had founded with his wife in 1905. It was during this period of intense reflection about the application of mathematics that Borel realized the role that the measure of randomness would be called upon to play in the future development of various sciences. Borel felt he was in possession of new tools to face mathematical questions, in particular the new analytical techniques of measure theory and Lebesgue integration. Also, risk quantification of life accidents was for Borel an essential information source for the organization of public social institutions : this was the core of the radical political program he was very close to. Borel therefore became one of the first mathematicians to engage in a renewal of probability theory. With the Great War, and his many involvements in the war effort, he began a career as a politician and used his position to become an active instigator of statistics based on probability. He thought that Paris needed an institute devoted to statistics, and convinced Lucien March and Fernand Faure to create such an institution, the ISUP. Georges Darmon started to teach in this institute. The ISUP was however only a place for lectures and in Borel's mind, probability and statistics also deserved a real research center. This led to the foundation of the Institut Henri Poincaré. Darmon and Fréchet, who left Strasbourg after Borel asked him to join the IHP team, decided to use the Institute as a hub where it would be possible to organize a kind of technology transfer for the importation of the methods of mathematical statistics that had been developed abroad (Britain, Scandinavia, Italy. . .). Darmon in particular was eager to introduce British mathematical statistics in France, firstly from Pearson's biometric school, and, at the end of the 1930s, from Fisher and his followers.

We tried to describe how the aforementioned transfer to the IHP was organized, from places where reflection on the mathematization of statistics had been pushed much farther than in France. It would be interesting to analyze more systematically the extent of simple reproduction and of transformation of the contents in this transfer along the lines proposed some years ago by Pestre about the reproduction of the experiments in physics. Pestre writes that *even in the case where [the scientist] chooses to reproduce an experiment to take over it and 'check' it, the reproduction is generally 'improved' and not 'identical' - and this often leads to complex debates about the relationship between these various experiments and the results proposed by them*<sup>63</sup>. *In our case, what should be focused on is the subsequent path of mathematical techniques with Anglo-Saxon or Scandinavian sources, to better understand how they became integrated (or not) into the mathematical statistics taught and 'produced' in France.*

63. Même dans le cas où il choisit de reproduire une expérience pour se la réapproprier et la "vérifier", la reproduction est en général "améliorée", et non "à l'identique" - ce qui conduit souvent à des débats complexes sur les rapports qu'entretiennent ces diverses expériences et les résultats qu'elles proposent ( [95], p.126).

Scrutinizing the *Annales de l'Institut Henri Poincaré* gives a good picture of how probabilistic statistics appeared in France between the two World Wars when German, Scandinavian and British statisticians came to the IHP to hold conferences. Darmois' and Guldberg's papers, which we have chosen as a sample because they were the first papers in statistics, provide a good illustration of the initial situation. A new generation of probabilists and statisticians obtained a doctorate at the IHP under the direction of Fréchet and Darmois at the end of the 1930s. Among them, two were more specifically oriented towards statistical topics : Malécot, whose research in biostatistics and genetics was based on Fisher's techniques, and Dugué, who became after the war the leader of French statistics at Paris University.

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